PROCEEDINGS

OF THE

PHYSICAL SOCIETY OF LONDON.

From December 1918 *to August* 1919.

VOL. XXXI.

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CONTENTS.

VOL. XXXI.

PROCEEDINGS

AT THE

MEETINGS OF THE PHYSICAL SOCIETY

OF LONDON.

SESSION 1918–1919.

October 25, 1918.

Meeting held at Imperial College of Science.

Prof. C. H. LEES, F.R.S., President, in the Chair.

A Discussion took place on "The Case for a Ring Electron," opened by Dr. H. S. ALLEN, M.A.

November 8, 1918.

Meeting held at Imperial College of Science.

Prof. C. H. LEES, F.R.S., President, in the Chair.

The following Papers were read :—

1. "Low-Voltage Arcs in Metallic Vapours." By Prof. J. C. MCLENNAN, F.R.S.

2. "Relativity and Gravitation." By Dr. W. WILSON.

A Demonstration of Experiments Illustrating Colour Blindness was given by Mr. C. R. GIBSON.

November 22, 1918.

Meeting held at Imperial College of Science.

Prof. C. H. LEES, F.R.S., President, in the Chair.

The following Papers were read :—

1. "Note on the Linguistic Nomenclature of Scientific Writers." By A. CAMPBELL, B.A.

2. "A Note on Low Frequency Microphone Hummers." By A. CAMPBELL, B.A.

3. "A Simple Tuning Fork Generator for Sine-Wave Alternating Current." By A. CAMPBELL, B.A.

4. "A Method of Comparing Tuning Forks of Low Frequency and of Determining their Damping Decrements." By A. CAMPBELL, B.A.

5.* "Cohesion" (Fifth Paper). By Prof. H. CHATLEY.

* Taken as read in absence of Author.

January 24, 1919.

Meeting held at Imperial College of Science.

Prof. C. H. LEES, F.R.S., President, in the Chair.

The following Papers were read :—

1. "Notes on Lubrication." By S. SKINNER, M.A.

2. "On Sir Thos. Wrightson's Theory of Hearing." By Prof. W. B. MORTON, M.A.

3. "Electrical Theorems in connection with Parallel Cylindrical Conductors." By Dr. A. RUSSELL, M.A.

Annual General Meeting.

February 14, 1919.

Mr. W. R. COOPER, M.A., in the Chair.

In opening the meeting the Chairman referred to the loss the Society had sustained by the death of Prof. G. Carey Foster, one of the Founders, and mentioned that the President, Prof. C. H. LEES, was representing the Society at the funeral that afternoon.

The Annual Report of the Council was read by Prof. ECCLES.

In the year 1918 twelve ordinary Meetings were held. The average attendance at the Meetings was 35.

The fourth Guthrie Lecture was delivered at the Meeting on March 22, 1918, by Professor J. C. McLennan, F.R.S. The subject of the lecture was " The Origin of Spectra."

The Annual Exhibition of Apparatus was again suspended owing to the War.

During the past session the Council resolved to devote some of the Meetings of the Society in each session to the discussion of selected subjects of current interest. This year a discussion on " The Teaching of Physics in Schools " attracted an audience of 101 Fellows and visitors.

In the autumn of 1917 the Council appointed a Committee to examine into the possibility of improving the Professional Status of the Physicist. Acting upon the report of this Committee, a Conference was arranged with representatives from other Societies, which has resulted in the proposal to establish an Institute of Physics of a professional type.

Professor C. H. Lees and Dr. S. W. J. Smith were nominated by the Council to serve on the Conjoint Board of Scientific Societies.

During the session the Society published a Report on the " Relativity Theory of Gravitation," by Professor A. S. Eddington, M.A., M.Sc., F.R.S.

The number of Honorary Fellows on the roll on 31st December, 1918, was 10, and the number of Ordinary Fellows 446. Twenty-seven new Fellows and one Student were elected during the year, and there was one resignation.

The Society has to record with regret the deaths of Mr. E. Russell Clarke, M.B.E., Mr James Enright, B.Sc., Professor O. Henrici, F.R.S., Professor B. Hopkinson, F.R.S., Mr. Sydney Lupton, M.A., and Sir H. T. Wood, B.A.

The Report of the Treasurer was read by Mr. W. R. Cooper.

The accounts for the past year show the position of the Society to be much sounder than in 1917, there being a credit balance of £98. 9s. 8d., as against a debit balance of £48. 17s. 9d. This is particularly satisfactory having regard to the fact that the expenditure was greater by £153. 9s. 2d. The heavier expenditure is due to the greatly increased cost of printing. The cost of printing and issuing the publications and notices amounted to £464. 10s. 8d., as compared with £384. 5s 4d. in the previous year; and, in addition, the Report on Relativity cost £77. 3s. 9d. In accordance with previous accounts, the publications have been taken over the complete volume (or session), which does not coincide with the financial year.

The revenue from subscriptions improved, and a considerable sum in arrears has been paid, though not as much as was hoped. Further, a sum of £70. 12s. 3d., due on account of income tax, has been recovered in respect of the two years 1916/17 and 1917/18.

The sale of publications has fallen off to a small extent, probably due to war conditions.

The balance-sheet shows that the outstanding subscriptions are still heavy, the amount being £180. 12s. It is doubtful if anything like this sum will be realised, and therefore a reserve of £80. 12s. has been set against this figure.

The investments, which have been valued at market prices through the courtesy of the London County, Westminster & Parr's Bank, have appreciated somewhat since the last balance-sheet. A sum of £100 has been invested during the year in National War Bonds.

Both Reports were unanimously adopted.

INCOME AND EXPENDITURE ACCOUNT.
From January 1st to December 31st, 1918.

Income.	£	s.	d.	£	s.	d.
Entrance Fees	28	7	0			
Subscriptions by Fellows	485	2	0			
" Voluntary	15	15	0			
" by Students	2	12	6			
" Arrears paid	78	0	6			
" Paid in Advance	11	7	6			
" for "Science Abstracts" and Advance Proofs	16	19	4			
				638	3	10
Composition Fees				63	0	0
Dividends :—						
Furness Debenture Stock	11	15	10			
Midland Railway	29	5	0			
Metropolitan Board of Works	5	1	6			
Lancaster Corporation Stock	8	14	0			
New South Wales	6	9	0			
London, Brighton & South Coast Railway	18	19	8			
Great Eastern Railway	14	15	0			
India $3\frac{1}{2}$% Stock	12	13	10			
Exchequer Bonds, 5%	20	0	0			
National War Bonds, 5%	1	14	9			
				129	8	7
Income Tax refunded				70	12	3
Interest on deposit account				6	1	0
Sales of Publications (Fleetway Press, Ltd.)				148	2	4
				£1,055	8	0

Expenditure.	£	s.	d.	£	s.	d.
"Science Abstracts"	277	12	0			
" " Extra copies	3	9	6			
				281	1	6
Fleetway Press, Ltd. :—						
"Proceedings"	345	16	4			
Bulletin	38	14	11			
Distribution (Postage)	37	12	10			
General	42	6	7			
				464	10	8
Report on Relativity				77	3	9
Reporting	43	16	8			
Refreshments and Attendance	15	0	10			
Guthrie Lecture—Honorarium	10	0	0			
Report on Relativity—Honorarium	20	0	0			
Royal Asiatic Society	2	2	0			
Petty Cash—						
Secretaries' Expenses	16	16	11			
Treasurer's Expenses	4	8	8			
Expenses in regard to Institute of Physics	4	0	1			
Bank Charges		2	3			
Insurance	6	10	0			
Advertising	1	5	0			
				124	2	5
Donation—Conjoint Board of Scientific Societies				10	0	0
Balance, being excess of income over expenditure				98	9	8
				£1,055	8	0

W. R. COOPER, *Honorary Treasurer.*

Audited and found correct,

T. MATHER
W. A. J. O'MEARA } *Honorary Auditors.*

January 28th, 1919.

BALANCE SHEET AT DECEMBER 31ST, 1918.

ASSETS.

	£	s.	d.	£	s.	d.
Subscriptions in arrears	180	12	0			
Less reserve for subscriptions probably unrealisable	80	12	0			
				100	0	0
Investments (valued at Dec. 31):—						
£533 Furness 3 per cent. Debenture Stock	287	0	0			
£1,600 Midland Railway 2½ per cent. Perpetual Preference Stock	744	0	0			
£200 Metropolitan Board of Works 3½ per cent. Consolidated Stock	173	0	0			
£400 Lancaster Corporation 3 per cent. Redeemable Stock	228	0	0			
£254 2s. 9d. New South Wales 3½ per cent. Ordinary Stock	23	0	0			
£500 London, Brighton & South Coast Railway Ordinary Stock	385	0	0			
£500 Great Eastern Railway 4 per cent. Debenture Stock	380	0	0			
£500 India 3½ per cent. Stock	347	0	0			
£400 Exchequer 5 per cent. Bonds, 1921	400	0	0			
£100 5% National War Bonds 1928	100	0	0			
				3,275	0	0
Outstanding Credit on Sales				11	2	9
Stock of Publications (Treasurer's valuation)				250	0	0
Cash at Bank on Deposit				50	0	0
Cash at Bank, Current Account at Dec. 31	101	10	1			
Adjustment for outstanding cheques	31	6	9			
				70	3	4
Cash in hand (Treasurer's Petty Cash)				1	16	8
				£3,758	2	9

LIABILITIES.

	£	s	d.
Life Compositions	1,892	0	0
Balance, General Fund	1,866	2	9
	£3,758	2	9

W. R. COOPER, *Honorary Treasurer.*

Audited and found correct,

T. MATHER
W. A. J. O'MEARA } *Honorary Auditors.*

January 28th, 1919.

LIFE COMPOSITION FUND AT DECEMBER 31ST, 1918.

	£	s.	d.
149 Fellows paid £10	1,490	0	0
3 Fellows paid £15	45	0	0
5 Fellows paid £21	105	0	0
8 Fellows paid £31. 10s.	252	0	0
	£1,892	0	0

W. R. COOPER, *Honorary Treasurer.*

January 28th, 1919.

Audited and found correct,

T. MATHER
W. A J. O'MEARA } *Honorary Auditors.*

After the customary votes of thanks, the election of Officers and Council took place, the new Council being constituted as follows :—

President.—Prof. C. H. Lees, D.Sc., F.R.S.

Vice-Presidents, who have filled the office of President.—Prof. R. B. Clifton, M.A., F.R.S.; Prof. A. W. Reinold, C.B., M.A., F.R.S.; Sir W. de W. Abney, R.E., K.C.B., D.C.L., F.R.S.; Prin. Sir Oliver J. Lodge, D.Sc., LL.D., F.R.S.; Sir R. T. Glazebrook, C.B., D.Sc., F.R.S.; Prof. J. Perry, D.Sc., F.R.S.; C. Chree, Sc.D., LL.D., F.R.S.; Prof. H. L. Callender, M.A., LL.D., F.R.S.; Prof. A. Schuster, Ph.D., Sc.D., F.R.S.; Sir J. J. Thomson, O.M., D.Sc., F.R.S.; Prof. C. Vernon Boys, F.R.S.

Vice-Presidents.—Prof. W. Eccles, D.Sc.; Prof. J. W. Nicholson, M.A., D.Sc., F.R.S.; Prof. O. W. Richardson, M.A., D.Sc., F.R.S.; R. S. Willows, M.A., D.Sc.

Secretaries.—H. S. Allen, M.A., D.Sc.; F. E. Smith, O.B.E., F.R.S.

Foreign Secretary.—Sir R. T. Glazebrook, C.B., D.Sc., F.R.S.

Treasurer.—W. R. Cooper, M.A., B.Sc.

Librarian.—S. W. J. Smith, M.A., D.Sc., F.R.S.

Other Members of Council.—Prof. E. H. Batron, D.Sc., F.R.S.; Prof. W. H. Bragg, C.B.E., M.A., F.R.S.; C. R. Darling, F.I.C.; Prof. A. S. Eddington, M.A., M.Sc., F.R.S.; D. Owen, D.Sc.; C. E. S. Phillips, F.R.S.E.; E. H. Rayner, M.A.; S. Russ, M.A., D.Sc.; T. Smith, B.A.; F. J. W. Whipple, M.A.

After the conclusion of the general business the chair was taken by the President, Prof. C. H. Lees. The following Papers were read :—

1. "The Temperature Coefficient of Tensile Strength of Water." By S. Skinner, M.A., and R. W. Burfitt, B.Sc.

2. "Vector Diagrams of Some Oscillatory Circuits used with Thermionic Tubes." By Prof. W. H. Eccles.

3. "A Small Direct-Current Motor using Thermionic Tubes instead of Sliding Contacts." By Prof. Eccles and Mr. F. W. Jordan.

February 28, 1919.

Meeting held at Imperial College of Science.

Prof. C. H. Lees, F.R.S., President, in the Chair.

The following Paper was read :—

"On Simplified Inductance Calculations, with Special Reference to Thick Coils." By Mr. P. R. Coursey, B.Sc.

A Demonstration of Some Acoustic Experiments in Connection with Whistles and Flutes was given by Dr. R. Dunstan.

A Demonstration of a New Polariser was given by Mr. G. Brodsky.

March 14, 1919.

Meeting held at Imperial College of Science.

Prof. C. H. Lees, F.R.S., President, in the Chair.

The following Paper was read :—

"Some Characteristics of the Spark Discharge and its Effect in Igniting Explosive Mixtures." By Messrs. C. C. Paterson and N. R. Campbell.

A Demonstration of the Uses of Invisible Light in Warfare was given by Prof. R. W. Wood.

March 28, 1919.

Meeting held at Imperial College of Science.

Prof. C. H. Lees, F.R.S., President, in the Chair.

A Discussion on "Metrology in the Industries" was held. Opener—Sir R. T. Glazebrook, C.B., F.R.S.

May 9, 1919.

Meeting held at Imperial College of Science.

Prof. C. H. Lees, F.R.S., President, in the Chair.

A Demonstration of a New Colour Transparency Process for Illustrating Scientific Lectures was given by A. E. Bawtree, F.R.P.S.

The following Papers were read :—

1. "Absolute Scales of Pressure and Temperature." By F. J. W. Whipple, M.A.

2. "On the Transmission of Speech by Light." By A. O. Rankine, D.Sc.

May 23, 1919.

Meeting held at Imperial College of Science.

Prof. C. H. Lees, F.R.S., President, in the Chair.

An Exhibition of a Tuning Fork maintained in vibration by means of a Triode Valve was given by Prof. W. H. Eccles.

The following Papers were read :—

1. "A Form of Knudsen's Vacuum Manometer." By Mr. L. F. Richardson.

2. "On Theories of Thermal Transpiration." By Mr. G. D. West.

June 13, 1919.

Special General Meeting, held at Imperial College of Science.

It was decided that the war-time practice of holding all meetings at 5 p.m. be continued.

It was proposed by the PRESIDENT and seconded by Mr. C. R. DARLING that the Society agree to the following proposals, made by the Institute of Physics :—

"The reduction of annual subscriptions by a member of any one Society on his joining a second Society (or more) shall be as follows for each of the Societies of which he is a member :—

$33\frac{1}{3}$ per cent. in the case of members of 4 or more Societies.
25 per cent. in the case of members of 3 Societies.
15 per cent. in the case of members of 2 Societies.

"The scheme would apply to persons already members of more than one Society as well as to new members. In the event of any person already a member of one or more Societies joining another Society of the group, the entrance fee usually charged by that Society shall be waived.

"A reduction of $33\frac{1}{3}$ per cent. in the published annual price of periodical publications shall be made by the Societies to members of any of the participating Societies."

The motion was carried unanimously.

After the general business the following Papers were read :—

1. *"A Comparison of the Wave-form of the Telephone Current Produced by a Thermal Detector and by a Rectifier in Heterodyne Reception." By Dr. BALTH. VAN DER POL, Junr.

2. "The Magnetic Properties of Varieties of Magnetite." By Prof. E. WILSON and Prof. E. F. HERROUN.

* Taken as read in the absence of the Author.

June 27, 1919.

Meeting held at Imperial College of Science.

Prof. C. H. LEES, F.R.S., President, in the Chair,

The following Papers were read :—

1. "The Current Voltage Characteristics of High-Voltage Thermionic Rectifiers." By Prof. C. L. FORTESCUE.

2. "On the Measurement of Small Susceptibilities by a Portable Instrument." By Prof. E. WILSON.

July 11, 1919.

Extra Meeting held at the National Physical Laboratory, Teddington, by invitation of the Director, Sir R. T. GLAZEBROOK, C.B., F.R.S

A number of demonstrations of work in progress at the Laboratory were given.

THE GUTHRIE LECTURE.

The Origin of Spectra.

By Prof. J. C. McLennan, *F.R.S., University of Toronto.*

Delivered March 22nd, 1918.

Since Bohr * presented his views in 1913 on the origin of radiation involving the conception of a nucleus atom with permanent and temporary, stationary or non-radiating electronic orbits within it there have probably been no advances made experimentally in the field of radiation which have been more stimulating or more far reaching in their consequences than the early experiments of Frank and Hertz.† In these experiments they showed that when heated mercury vapour was traversed by electrons possessing kinetic energy acquired in passing through a fall of potential of about 4·9 volts, the vapour was stimulated to the emission of the single wave length $\lambda=2536{\cdot}72$ Å.U. This result constituted a new and, in so far as range of spectrum involved is concerned, most interesting application of the quantum theory, for it will be seen that with the relation $Ve=h\nu$, where $h=6{\cdot}6\times 10^{-27}$ erg. sec., 4·9 volts is the potential fall which corresponds to the frequency of the wave length $\lambda=2536{\cdot}72$ Å.U. Interest was added to this discovery when Frank and Hertz also pointed out that they were led by their experiments to the view that when mercury vapour was bombarded by electrons the minimum energy required to ionise a mercury atom was that acquired by an electron in a field of 4·9 volts difference of potential. It was soon seen that these two results were not compatible with the theory of Bohr of the origin of radiation, for assuming the validity of the quantum theory, it is impossible for an atom which possesses a series of stationary orbits to be ionised, and, being ionised, to be capable of emitting a radiation consisting of a single wave length. Apart from this consideration, however, Frank and Hertz's experiments were interesting and stimulating in that they directed attention to certain fundamental series in the spectra of the elements and indicated the possibility of connecting up quantitatively certain numerical relationships associated with

* Bohr, "Phil Mag.," July, Sept. and Nov., 1913; March, 1914; Feb. and Sept., 1915.

† Frank and Hertz, "Verh." d. Deutsch. Phys. Ges., Vol. X., p. 457.; and Vol. XI., p. 512.

these series with the dimensions and other features of the stationary electronic orbits of the atoms themselves. These remarkable experiments of Frank and Hertz have formed the starting point of a rather elaborate piece of experimental work, in which it has been my privilege in collaboration with my students to take a part. In the communication which is to follow it will be my endeavour to outline the main features of the development which has taken place in the interval, and to indicate, if possible, the significance and trend of the investigation to date.

(1.) *Origin of Radiation and the Quantum Relation.*

Following the original experiments of Frank and Hertz and extending them to include vapours other than mercury, we have been able to show that when the vapours of mercury, zinc, cadmium and magnesium * were bombarded by streams of electrons of moderate intensity from an incandescent limed cathode, as the mean kinetic energy of the electron was increased a point was reached when the respective vapours emitted a radiation of but a single and definite wave length. With mercury this wave length was $\lambda=2536 \cdot 72$ Å.U., with zinc it was $\lambda=3075 \cdot 99$ Å.U., with cadmium $\lambda=3260 \cdot 17$ Å.U., and with magnesium $\lambda=2852 \cdot 22$ Å.U. Moreover, the impact voltages which were found to be requisite for the stimulation of these monochromatic radiations were those which are given by the quantum relation $Ve=h\nu$, ν being the frequency of the radiation emitted.

In these experiments it was also found that no additional radiation was emitted by the vapours as the electrons were speeded up until arcs were struck, which they were when the impact voltages applied were those given by the quantum relation $Ve=h\nu$ for the frequency $\nu=(1 \cdot 5, S)$. This frequency it will be noted, was the frequency of the shortest wave length of the series represented by $\nu=(1 \cdot 5, S)-(m, P)$ in the spectrum of each of the vapours mentioned above.

This work has since been extended by Bergen Davis and Goucher,† who have shown, when using mercury vapour of low density, bombarded by electrons, that while a radiation of wave length $\lambda=2536 \cdot 72$ Å.U. was emitted *without ionisation*

* McLennan and Henderson, "Proc." Roy. Soc. A, Vol. XCI., p. 485 McLennan, "Proc." Roy. Soc. A., Vol. XCII., p. 307; McLennan, "Phys. Rev.," Vol. X., p. 84, July, 1917.

† Bergen Davis and Goucher, "Phys. Rev.," Vol. X., No. 2, p. 101, 1917.

when a mean impact voltage of 4·9 volts (the quantum relation voltage) was used, an additional radiation of wave length $\lambda=1849$ Å.U. appeared when the mean applied impact voltage was 6·7 volts. For this radiation it will be seen the quantum relation $Ve=h\nu$ again applied. They also showed that ionisation by impact without an apparent increase in radiation occurred with an impact voltage of about 10·4 volts. This voltage, it will again be noted, corresponds by the quantum relation to the frequency of the shortest wave length of the principal series $\nu=(1{\cdot}5, S)-(m, P)$.

It follows, therefore, that the arcing potential with mercury vapour under bombardment by streams of electrons of moderate intensity is the same as the ionisation potential for mercury atoms, and that both these magnitudes are given by the quantum relation using the frequency $\nu=(1{\cdot}5, S)$. This result, therefore, confirms the view originally put forward by me,* which was also put forward independently by Bohr about the same time in so far as the relation of the ionisation potential to the spectral frequency $\nu=(1{\cdot}5, S)$ is concerned.

The results obtained by Bergen Davis and Goucher, who used the photo-electric effect for the detection and identification of the radiations emitted, have recently been extended to the vapours of zinc and cadmium by Mr. Ireton † and myself, using the photographic method. With zinc vapour it has been found that by the use of quantum impact voltages it is possible to stimulate the radiations of wave length $\lambda=3075{\cdot}99$ Å.U. and $\lambda=2139{\cdot}33$ Å.U., and with cadmium vapour the radiations of wave length $\lambda=3260{\cdot}17$ Å.U. and $\lambda=2288{\cdot}79$ Å.U. In the case of both vapours no indication of the existence of the shorter wave length was obtained on the plates until the impact voltage demanded by the quantum relation for it was applied. With impact voltages applied higher than those corresponding to the above mentioned shorter wave lengths no additional radiation was obtained until arcs were struck. In so far, then, as our own experiments and those of Bergen Davis and Goucher go it is clear that they seem to indicate that by the use of "quantum" determined impact voltages it is possible to make the vapours of mercury, zinc and cadmium emit radiation consisting of one, and only one, wave length, and that by the use of still

* McLennan and Henderson, "Proc." Roy. Soc. A., Vol. XCI., p. 490, 1915.

† McLennan and Ireton. Communicated to the "Phil. Mag.," Oct., 1918.

higher "quantum" impact voltages to make the vapours emit a radiation consisting of two, and only two, wave lengths. Fig. 1, which is typical of all the vapours referred to, shows the results obtained with zinc. Reproduction No. 1 shows the many lined spectrum of the zinc spark. Reproduction No. 2 shows the line at $\lambda=3075{\cdot}99$ Å.U., which was brought out when an impact voltage of 5·6 volts was used, and reproduction No. 3 shows both the lines corresponding to $\lambda=3075{\cdot}99$ and $\lambda=2139{\cdot}33$ Å.U., and was obtained with an impact voltage of 7·5 volts. As 5·8 volts is the quantum voltage for $\lambda=2139{\cdot}33$ Å.U. and 4 volts that for $\lambda=3075{\cdot}99$ Å.U., it will be seen that when an applied P.D. of 5·6 volts was used we were well above that demanded for the longer wave length, and just below that required by the quantum theory to bring

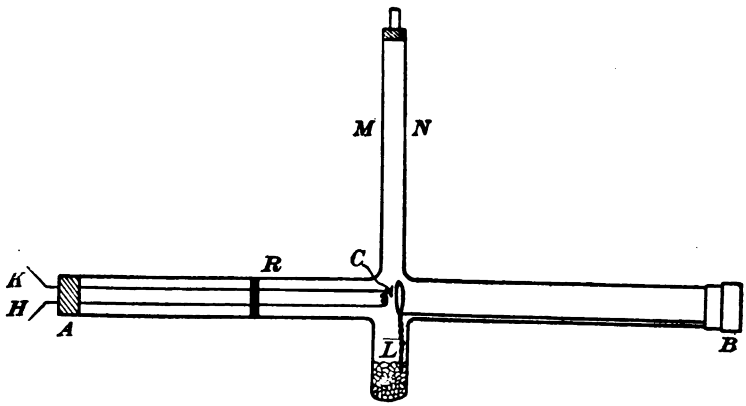


FIG. 2.

out the shorter wave length. It is interesting to record that even with exposures of 10 hours' duration no indication was obtained on the photographic plates of the wave length 2139·33 Å.U., when an impact voltage of 5·6 volts was used. The form of the fused quartz tube used in carrying out these experiments is shown in Fig. 2, and has been fully described elsewhere. *C* was an incandescent tungsten cathode, and *L* was the zinc metal which was vaporised. It should also be stated that in measuring the impact voltage allowance was made for the drop in potential over the incandescent filament and its leads.

TABLE 1.

Series Spectra.

$(1 \cdot 5, S)-(m, p_2)$ series.

Mercury.		Zinc.		Cadmium.		m.
2536·72	....	3075·99	....	3260·17	....	2
1435·59	....	1632·08	....	1710·58	....	3
1307·83	....	1468·90	....	1537·89	....	4
1259 31	....	1408·86	....	1474·06	....	5
1235·91	....	1379·38	....	1442·60	....	6
1222·44	....	1362·59	....	1424·40	....	7
1213·97	....	—	....	—	....	8
—	....	—	....	—	....	—
—	....	—	....	—	....	—
Lt 1188·00	....	1320·00	....	1378·7	....	∞

$(1 \cdot 5, S)-(m, P)$ series.

Mercury.		Zinc.		Cadmium.		m.
1849·6	....	2139·33	....	2288·79	....	2
1402·71	....	1589·64	....	1669·3	....	3
1268·9	....	1457·64	....	1526·73	....	4
1250·6	....	1376·97	....	1469·35	....	5
—	....	—	....	—	....	—
Lt 1188·0	....	1320·0	....	1378·7	....	∞

From Table I. it will be noted that the wave lengths $\lambda=2536 \cdot 72$ Å.U., $\lambda=3075 \cdot 99$ Å.U., and $\lambda=3260 \cdot 17$ Å.U. are respectively the first members of the combination series $\nu=(1 \cdot 5, S)-(m, p_2)$ and the wave lengths $\lambda=1849 \cdot 6$ Å.U., $\lambda=2139 \cdot 33$ Å.U., and $\lambda=2288 \cdot 79$ Å.U., the first members of the singlet principal series $\nu=(1 \cdot 5, S)-(m, P)$. The other members of both these series for the three metals, as the table shows, are all beyond the range of wave lengths which can be recorded by a spectrograph with an optical train ot quartz. It would be interesting to extend the experiments described above so as to see if the higher members of these two series would come out on the plate one by one if the impact voltage of the electrons were increased to that given by the quantum relation for their frequencies. To do this it would be necessary to use a fluorite spectrograph or a vacuum grating spectrograph for the range intermediate between $\lambda=1900$ Å.U. and $\lambda=1400$ Å.U., and a vacuum grating spectrograph for the range below $\lambda=1400$ Å.U.

To work in this direction some experiments were set on foot for me by Mr. Ainslie with a fluorite spectrograph and others by Mr. Lang with a vacuum grating spectrograph. It was found that with both instruments much time was consumed in working out technical details. The results obtained to date with them will be published shortly, and they will show that with the fluorite spectrograph it is now easy to obtain

spectrograms down to $\lambda=1400$ Å.U. With the vacuum grating spectrograph spectrograms well below $\lambda=600$ Å.U., can be readily obtained. Through time devoted to war work, however, the experiments had to be discontinued before it was possible to examine with care the point raised above.

(II.) *Investigation of the Ultraviolet Region with a Fluorite Spectrograph.*

The general characteristics of the fluorite spectrograph will readily be seen from the sketch shown in Fig. 3, and the instrument as in actual use is shown in the photographic reproductions of Fig. 4 and Fig. 5. The prism was mounted in a central brass chamber from the two opposite sides of which projected a brass collimator tube and a brass tube terminated by a plate holder. This latter consisted of a shallow rectangular box carrying a frame which could be slightly rotated when adjustment was necessary. The box was provided with an opening at one end through which the photographic plate could be slipped into position. When the plate was in position and ready for an exposure the opening was closed with a small brass cap, and the joint made airtight by means of a rubber washer. The operation of inserting the plate and of closing the opening in the plate-holder was, of course, carried out in the dark. The photographic plates used were narrow strips cut from Schumann plates recently prepared and put on the market by the Adam Hilger Co.

In the course of the experiments it was found necessary to remove all water vapour from the spectrograph, in order to secure the best photographic results in the region of the shorter wave lengths. For that purpose a large cylindrical brass drying chamber, shown in Fig. 3, was attached to the collimator tube. Through an opening in one end of this chamber it was possible to insert or remove when desired a shallow tray filled with phosphorus pentoxide. The plate *P*, Fig. 3, provided with a rubber washer, enabled one to close the opening quite satisfactorily. The collimator tube was provided with two closely fitting tubes *A* and *B*, concentric with it. One of these, *A*, carried a disc, in which was inserted the condensing fluorite lens and the other, *B*, carried a plate to which the slit jaws were attached. The focussing lens was carried on a disc borne by a sliding tube, *E*, similar to *A* and *B*. The edges of the discs which carried the lenses were pierced

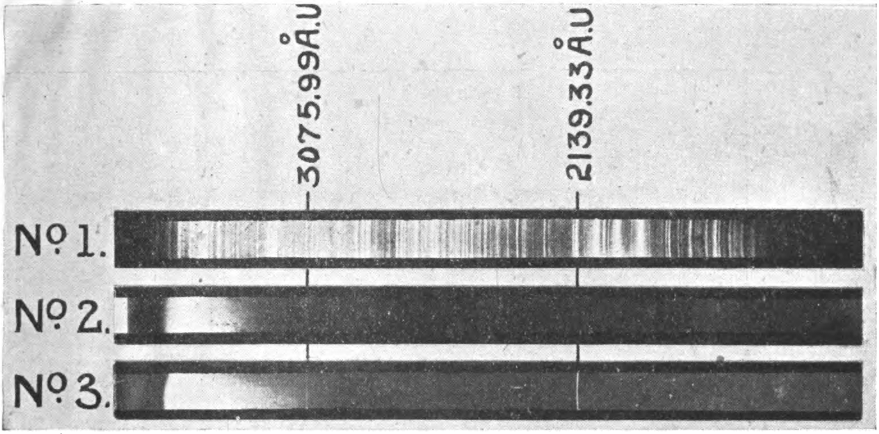


Fig. 1

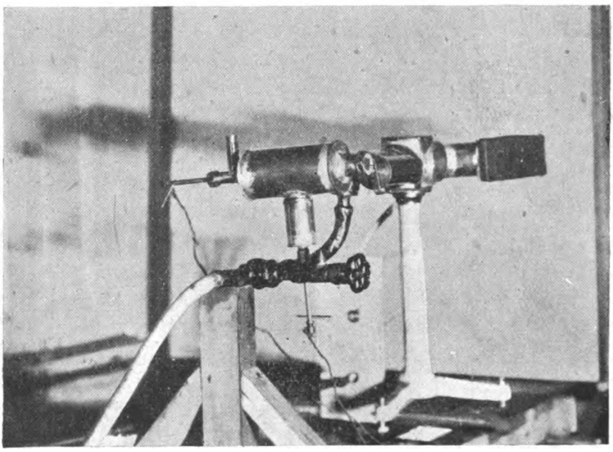


Fig. 4.

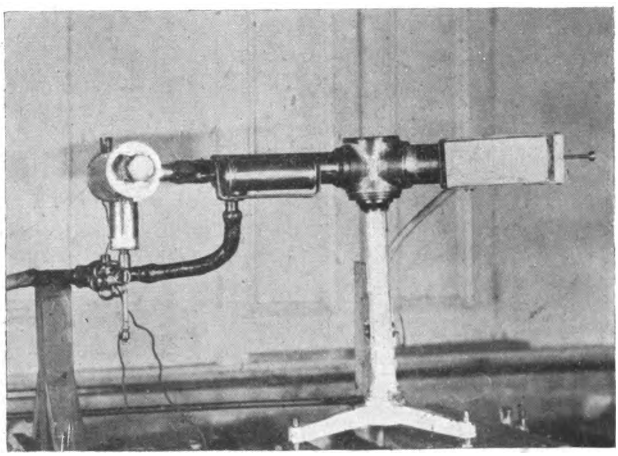


Fig 5.

[To face p. 6.

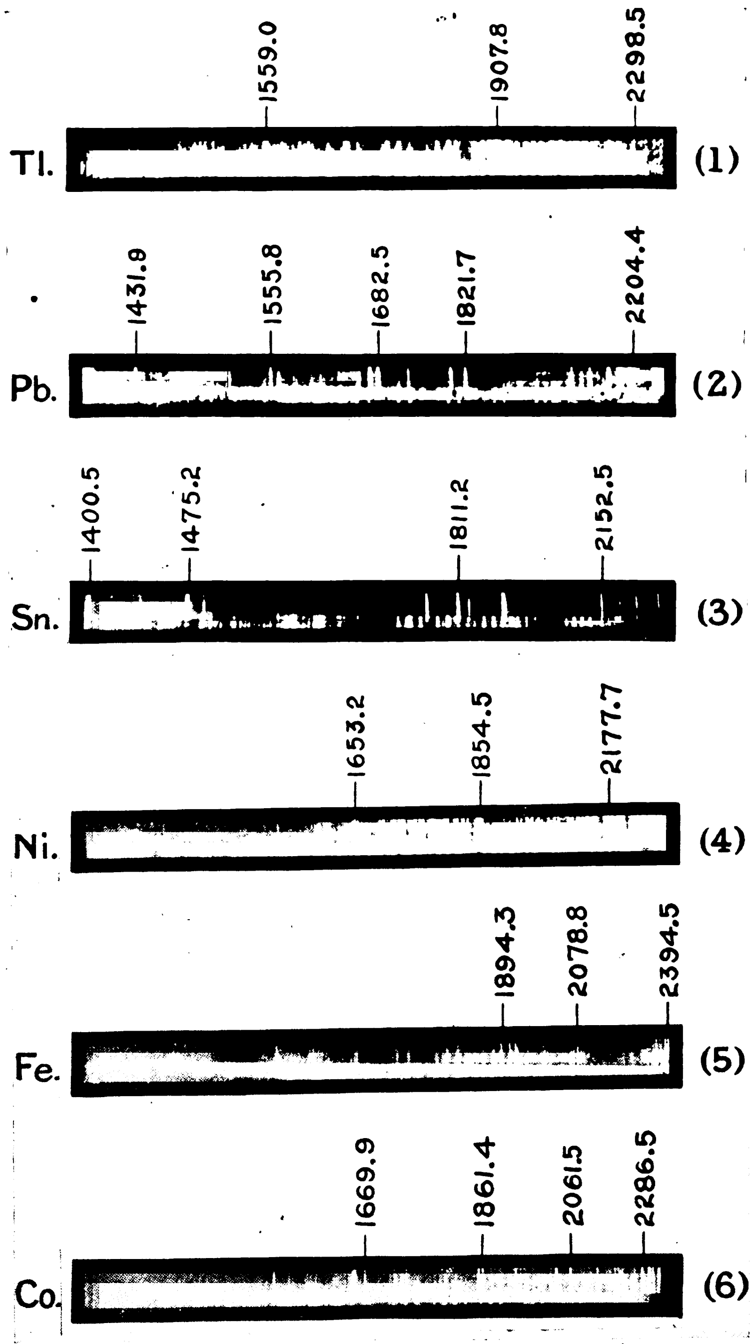

Tl.
1559.0
1907.8
2298.5
(1)
Pb.
1431.9
1555.8
1682.5
1821.7
2204.4
(2)
Sn.
1400.5
1475.2
1811.2
2152.5
(3)
Ni.
1653.2
1854.5
2177.7
(4)
Fe.
1894.3
2078.8
2394.5
(5)
Co.
1669.9
1861.4
2061.5
2286.5
(6)

FIG. 6.

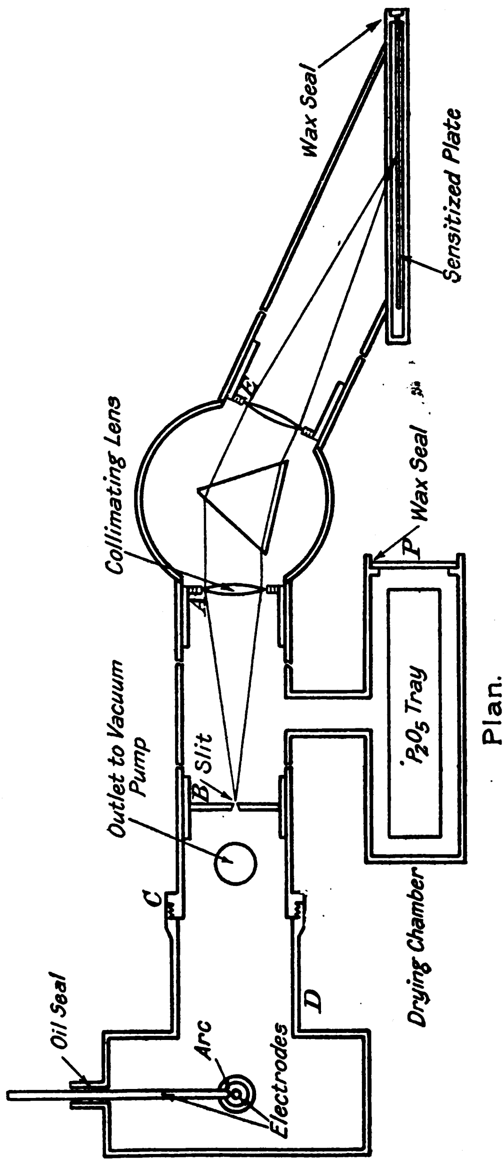

Wax Seal
Sensitized Plate
E
Collimating Lens
A
Wax Seal
P
Outlet to Vacuum Pump
B
Slit
C
D
Oil Seal
Arc
Electrodes
Drying Chamber
P_2O_5 Tray
Plan.

Fig. 3.

with a number of holes so that the air had free access through them to all parts of the instrument. The air was removed from the apparatus by means of a set of oil-sealed mechanical Trimount pumps, the connections being made with lead piping at the points shown in Fig. 3. The reproductions shown in Fig. 6 will serve to show the character of the results obtainable with arcs in vacuum between electrodes of thallium, lead, tin, nickel, iron and cobalt. The hydrogen spectrum is shown accompanying each reproduction. From the spectrograms it will be seen that wave lengths down to $\lambda=1400$ Å.U. were readily recorded.

(III.) *Investigation of the Extreme Ultraviolet Region with a Vacuum Grating Spectrograph.*

The vacuum grating spectrograph was designed by and obtained from the Adam Hilger Co. A sectional plan and sectional elevation of this instrument are shown in Fig. 7. Sketches of the grating mounting are shown in Fig. 8 and others of the slits, slit taps, auto-collimating eye-piece and film-carrying tap are given in Fig. 9.

The various parts with their designating letters are as follows :—

A Tinned brass tube.
B. Ground end plate.
C, Grating carriage.
D. Rails for carriage.
E. Ruled diffraction grating: 20,000 lines per inch, 120 cm. radius.
F. Tube with ground fitting for exhaust.
G. Large ground tap containing film carrier.
H. Photographic film, Schumann sensitised.
I. Auto-collimating eye-piece.
J. Glass right-angled prism.
K. Slit.
L. Exhaust tube for film carrier.
M. Aperture for loading film carrier.
N Ground fitting for eye-piece, replaced by ground plug in working.
O. Ground taps for exposing through slit.
Q. Screw for adjusting vertical inclination of grating.
R. Micrometer screw for setting orientation of grating.
S Mount for diffraction grating.

The spectrograph was formed of a brass tube (*A*) 115 cm. long, 15 cm. in diameter and 3·5 mm. thick. At one end there was a cover (*B*) ground in and soldered and at the other a casting containing the slit and film holder. The slits (*K*), of which there were two 5·5 cm. apart, were mounted in a recess in the casting, and kept airtight when desired by a window. Each slit was provided with a tap (*O*) to enable

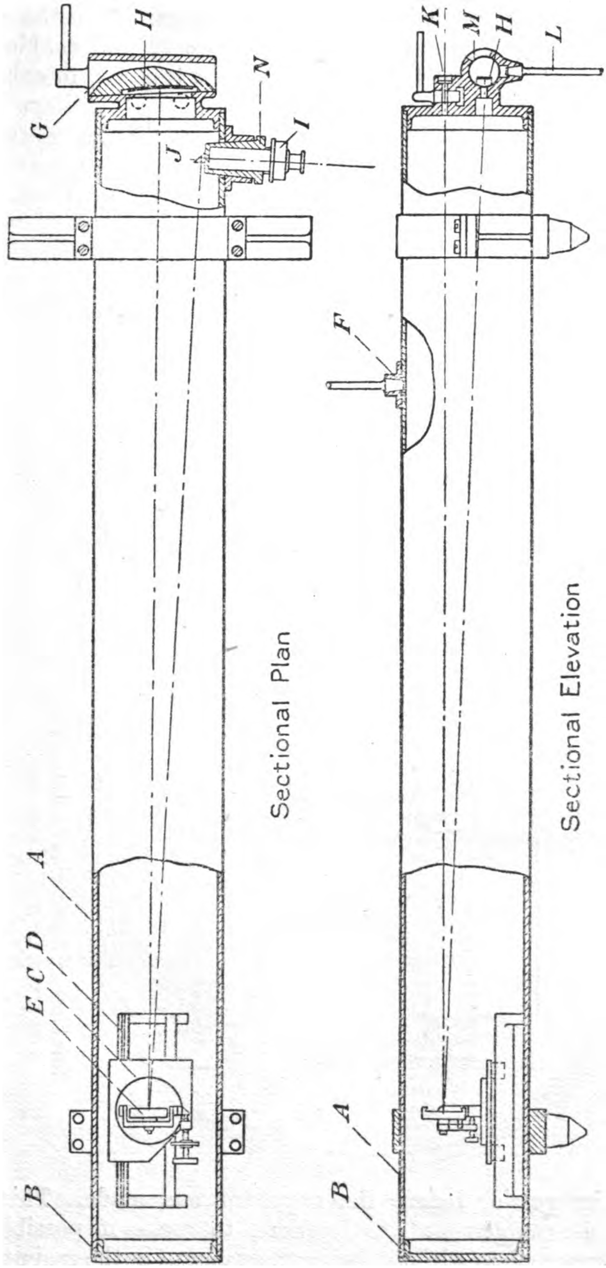
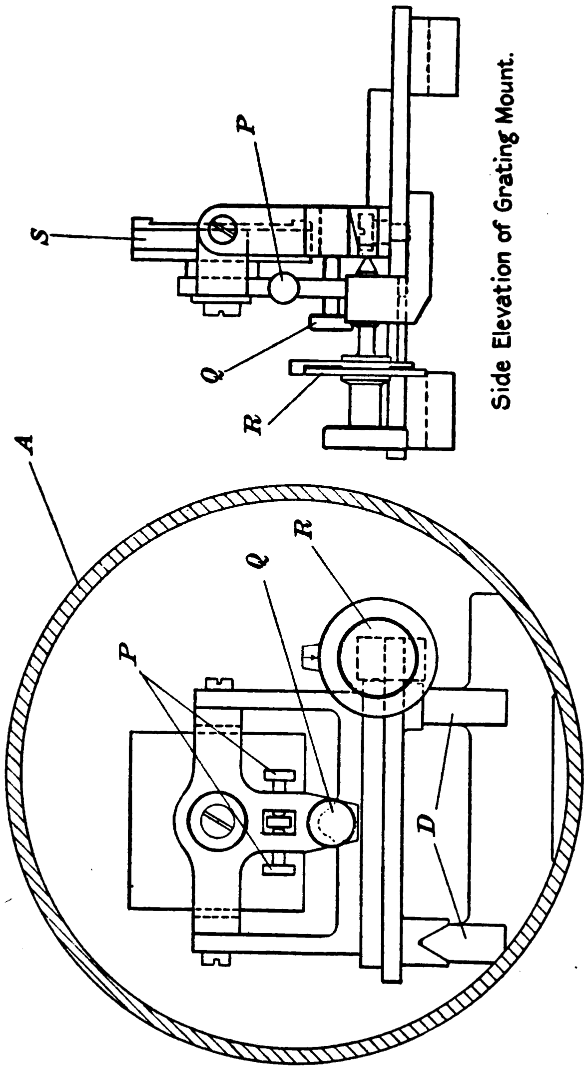


FIG. 7.

exposures to be made. The film (H) was carried in the main tap (G). The tap body had an aperture (M) to enable the film carried to be loaded, and an exhaust tube (L) to exhaust



Side Elevation of Grating Mount.

Section of Spectrograph looking towards Grating Mount.

FIG. 8.

the film carrier before the exposure was made. This ingenious arrangement was intended to make it possible to introduce a plate without losing the vacuum in the main tube.

At the slit and film end of the tube on one side there was a mount for an auto-collimating eye-piece (*I*) and prism (*J*) mounted in a taper plug (*N*). When not in use the plug (*N*) with eye-piece and prism were withdrawn complete and replaced by a solid plug.

At the other end of the tube were rails (*D*) on which slid a carriage (*C*) carrying the grating mount (*S*). The length of

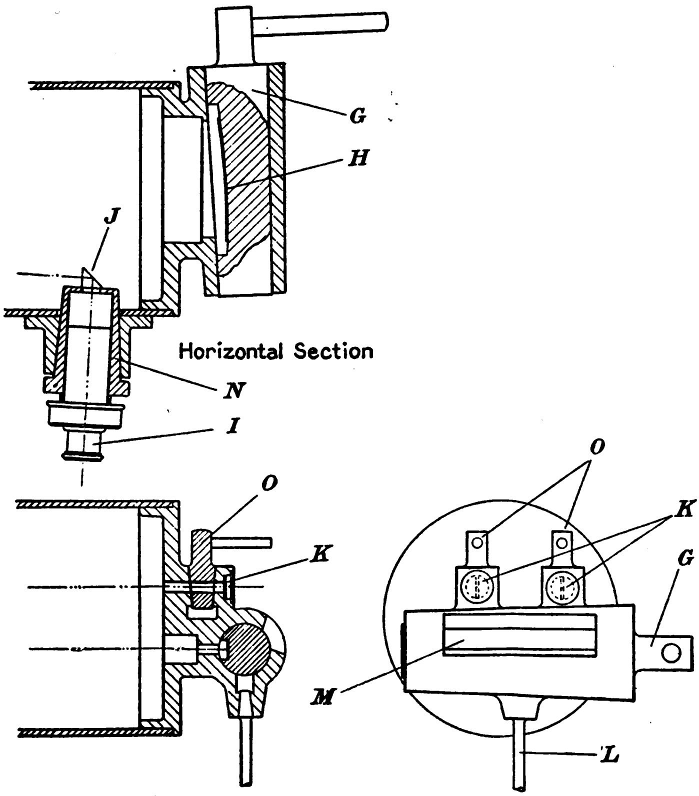


Fig. 9.

rails was sufficient to allow about 10 cm. travel for the carriage. The ruled grating No. 106 was 2·5 cm. wide and 1·9 cm. high. Its radius of curvature was 120 cm. and its ruling consisted of 20,000 lines to the inch. The mount was provided with adjusting screws (*P*) for setting the lines of the grating parallel to the slit, an adjusting screw (*Q*) for vertical

inclination of grating, and a micrometer screw for setting orientation of grating. The tilt of the grating and also of the film was 3° 13′.

The main exhaust pipe (*F*) was situated near the slit and film end of the tube and the volume to be exhausted was about 23·5 litres.

The inside of the main tube and all brass work in its interior were tinned; in the case of ground fittings, such as the taps (*G*) and (*O*), the grinding was done after tinning.

It was found exceedingly difficult to secure complete absence of porosity by tinning, and in the end this was accomplished by painting the whole of the exterior of the main tube with a thin layer of heated Chatterton's compound. In the course of the work it was found to be an advantage to paint the whole of the interior surface of the large tube a dull black to prevent reflections, and a number of diaphragms were placed at equal distances throughout its length for the same purpose.

A cylinder made of brass tubing 30 cm. long and 10 cm. in diameter, and not shown in the drawings, was connected to the main spectrograph tube by means of a short tube of large diameter. Through an opening in the end of this smaller cylinder a tray containing a thin layer of P_2O_5, and covered with glass wool could be inserted or withdrawn as desired. To secure the best results it was found necessary to remove all water vapour from the interior of the apparatus. Before making exposures with the arrangement described considerable quantities of phosphorus pentoxide were used, and this removed the vapour very rapidly when each exhaustion was made. The opening through which the tray was inserted was closed with a brass plate and made airtight with the Chatterton compound.

The photographic plates used were of the Schumann type, manufactured by the Adam Hilger Co. They were cut as desired from larger plates, and were about 5 mm. in width and 8 cm. in length. About 700 Å.U. was the range which it was possible to investigate with one slit without shifting the grating. The plate holder was of such a construction that the thin plates when inserted in the holder were bent to an arc of the circle, which included the ruling of the grating.

In making the exhaustions two Trimount oil pumps in series, run at 250 revs. per min. were used. To test the character of the vacuum obtained from time to time, a short

glass vacuum tube was sealed by a side tube into an opening made in the side of the spectrograph main tube. A discharge from a small induction coil was sent at intervals through the vacuum tube, and the appearance of the discharge gave an indication of the pressure within the apparatus.

Connection from the spectrograph to the pumps was made through a lead pipe of about 1 in. diameter provided with a $\frac{7}{8}$ in. Jenkins disc steam valve. This steam valve, which was provided with an oil seal as well as the usual stuffing box arrangement around the stem, worked very satisfactorily.

In practice it was found impossible to keep the ground joint made by the tap *G* with its matrix perfectly airtight at every point without coating all exposed parts of the joint with wax. In turning the tap to expose a plate this wax coating was always broken, with the result that leaks occurred. After spending considerable time trying to rectify this defect, the best results were finally obtained by abandoning the idea of

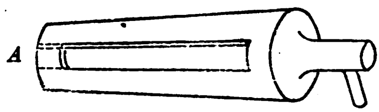


FIG. 10.

exhausting the plate-holder space in the tap *G* preliminary to turning the latter, so as to expose the plate.

The tap *G*, as originally provided with the apparatus, was replaced by a second one shown in Fig. 10. This large tap carried the plate holder as the original one did, and instead of the plate holder being inserted in a recess in the side of the tap as formerly the tap was slotted out to one end as shown at *A* in Fig. 10. This allowed the plate holder mounted on a thin brass strip to be inserted from the end *A*. When the plate was inserted the opening was covered with a small brass disc, which was sealed about its edge with wax. With this arrangement the tap always had its plate-holding recess facing the grating and ready for an exposure. The exposed parts of the tap joint were permanently covered with a wax coating to secure air-tightness.

The use of the special tap necessitated the re-exhausting of the whole apparatus, each time an exposure was made. The Trimount pumps, however, were very rapid and efficient in action, and it was found comparatively easy to reach

with half an hour's continuous running, a pressure low enough for the induction coil discharge to give green fluorescence in the pressure testing vacuum tube. The reproductions shown in Fig. 11 serve to indicate the results obtainable with the instrument. The preliminary photographic work was chiefly done with arcs in vacuum between carbon terminals, and, as the reproductions show, the spectrum of this element was investigated as far down as 584 Å.U.

(IV.) *Ionisation Potentials.*

Referring again to Frank and Hertz's experiments, it will be recalled that they found that when mercury vapour was bombarded by electrons with a mean impact voltage of 4·9 volts, it became conducting, and they concluded from this that 4·9 volts was the ionisation potential for mercury atoms. They also found, as stated above, that the same impact voltage just sufficed to stimulate the vapour to the emission of monochromatic radiation of wave length $\lambda = 2536{\cdot}72$ Å.U., which latter is in accordance with the requirements of quantum theory. If, however, the Bohr theory, that radiation of a definite frequency is emitted by an electron as it returns from an outer stationary orbit to an inner one, be accepted as the explanation of the origin of the emission of radiation giving a line spectrum by a nucleus atom of the Rutherford type, then Frank and Hertz's interpretation of their experiments leads us into difficulty, for it is impossible to hold Bohr's theory of the origin of radiation to be true, including acceptance of the quantum principle, and at the same time to hold the view that impact voltages which would just stimulate atoms to the emission of radiation consisting of one wave length will also suffice to ionise these atoms. An atom which is ionised is one which has had one or more of its electrons removed beyond its outermost stationary orbit. Consequently, if an atom be ionised it is in a condition to emit not a single line spectrum, but a spectrum consisting at least of one whole series of lines. In my early experiments I pointed out that my experiments led me to the view that if ionisation of mercury vapour was really brought about with an impact voltage of 4·9 volts, then an impact voltage of about 10·4 volts produced a *second* type of ionisation. I also pointed out that this type of ionisation was in accordance with the Bohr theory of the origin of radiation, because an impact voltage of 10·4 volts was just sufficient to establish an arc in the vapour, and so bring out the

many lined spectrum of the element. As already pointed out, the matter has been cleared up to a certain extent by the experiments of Bergen Davis and Goucher, for these experimenters have shown that the real ionisation potential of mercury vapour is about 10·4 volts, and that the conductivity which Frank and Hertz observed, with impact voltage of 4·9 volts, was not due directly to the bombarding electrons, but was due to the photo-electric reaction of the radiation of wave length $\lambda=2536{\cdot}72$ Å.U. stimulated by the electric bombardment of the vapour. Bergen Davis and Goucher take the view that this photo-electric conductivity has its origin at the surface of the metallic electrodes upon which the radiation fell. It may conceivably, too, have had an origin, in part, at least, in condensed layers of mercury vapour in the neighbourhood of the cathode, for Derieux * has recently shown that the long wave length limit for the production of the photo-electric effect with mercury droplets is greater than $\lambda=2536{\cdot}72$ Å.U. and less than $\lambda=3126$ Å.U., and is probably, therefore, about $\lambda=2800$ Å.U.

Tate † and Bishop ‡ have also recently confirmed experimentally the results of Bergen Davis and Goucher, and it now appears to be settled that 10·4 volts is the real ionisation potential of mercury atoms. It follows, therefore, that we have in this conclusion regarding the ionisation potential for mercury strong support for the view put forward by me that impact voltages which just suffice to establish arcs in metallic vapours when electronic streams of moderate density are used are exactly the same in magnitude as the ionisation potentials for the atoms of these vapours.

Going one step further, we see that since with mercury, zinc, cadmium and magnesium vapours the arcing potentials with streams of electrons of moderate density have been shown to be practically identical with the P.D.s given by the quantum equation for the frequencies $\nu=(1{\cdot}5,\ S)$ of the spectra of these vapours, it follows that of all the wave lengths in the spectra of at least a number of the elements those whose frequencies are given by $\nu=(1{\cdot}5,\ S)-(m,\ P)$, and possibly, too, by $\nu=(1{\cdot}5,\ S)-(m,\ p_2)$, are of fundamental importance. A knowledge of the wave lengths of these in the spectrum of an element, enables us to calculate the frequency $\nu=(1{\cdot}5,\ S)$ for

* Derieux, " Phys. Rev.," Vol. XI., No. 4, p. 276, April, 1918.
† Tate, " Phys. Rev.," Vol. X., No. 1, p. 81, July, 1917.
‡ Bishop, " Phys. Rev.," Vol. X., No. 3, p. 244, Sept., 1917

that element, and consequently enables us to calculate the ionisation potential for it. We can, therefore, follow any one of three paths in determining the ionisation potential of an element, namely, (1) by determining the minimum arcing potential for its vapour, (2) by using a special electrical method, such as that designed by Bergen Davis and Goucher, or (3) by determining for the element the wave lengths in its spectrum, which constitute the series $\nu=(1{\cdot}5, S)-(m, P)$.

The first method, as will be seen later, is apt to lead us into error, for the conditions which should be fulfilled in experimentally determining minimum arcing potentials are not so well defined as was at first supposed. The second method can only be used with gases or with metals which can be vaporised at comparatively low temperatures. The third method is probably the one which admits of widest application. An attempt should therefore be made to identify the wave lengths of the series $\nu=(1{\cdot}5, S)-(m, P)$ in the spectra of as many of the elements as possible. Up till recently the series was known only for mercury, zinc, cadmium, magnesium, calcium and strontium. For many of the elements the series, or a majority of the members of it, lies in the ultra violet or in the extreme ultra violet region of the spectrum, and consequently the investigation is fraught with some difficulty. If the requisite optical apparatus be available one method to follow is to use the Zeeman effect to identify the series. Another method which may be used is to take advantage of the phenomenon of absorption. For the elements for which the series is known the wave lengths constituting the series $\nu=(1{\cdot}5, S)-(m, P)$ are strongly absorbed by the non-luminous vapour of these elements. It may be that this characteristic is fundamental and a property of the elements generally. If it should turn out to be so the utilisation of absorption would appear to be the easiest method of spotting the series. Quite recently, in collaboration with Mr. J. F. T. Young,* I found the method applicable not only for calcium and strontium, but for barium as well. In the application of the method it was found that if a small quantity of the metal or of one of its salts were vaporised in a carbon arc, it was possible to obtain reversals with certain densities of vapour at only those wave lengths which constituted the $\nu=(1{\cdot}5, S)-(m, P)$ series. Reproductions showing the results obtained are given in Figs. 12, 13

* McLennan and Young. Communicated to the Royal Society, Oct., 1918.

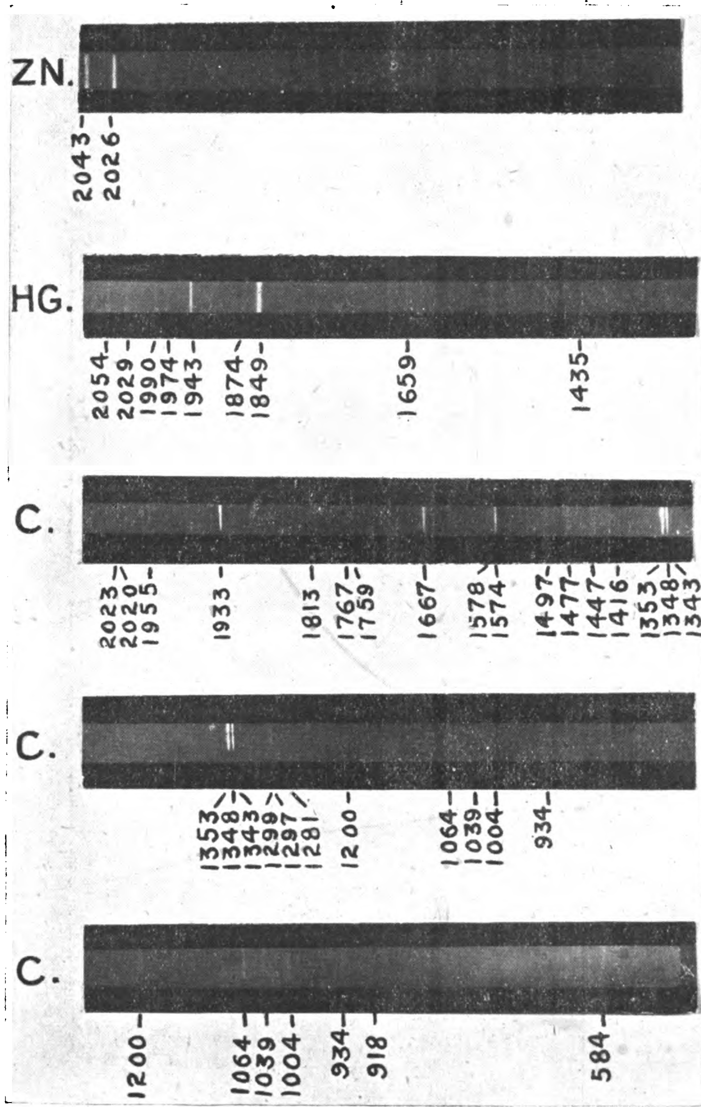


Fig. 11.

[*To face p. 16.*

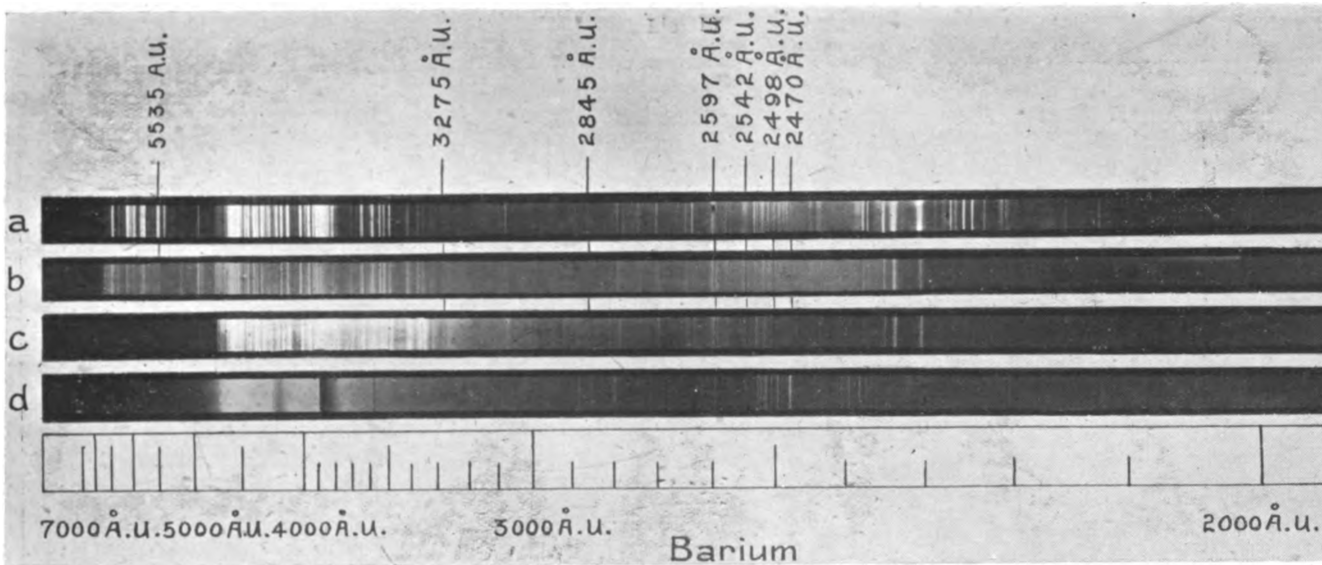

5535 A.U.
3275 Å.U.
2845 Å.U.
2597 Å.U.
2542 Å.U.
2498 Å.U.
2470 Å.U.
a
b
c
d
7000 Å.U.
5000 Å.U.
4000 Å.U.
3000 Å.U.
2000 Å.U.
Barium

Fig. 14.

and 14. In each set of reproductions (*a*) is the emission spectrum of the metal taken with a Schumann plate, (*b*) is the absorption spectrum of the metal taken on a panchromatic plate, (*c*) is its absorption spectrum taken with a Schumann plate, and (*d*) is the emission spectrum of the carbon arc alone.

From these experiments the wave lengths of the series $\nu=(1\cdot5, S)-(m, P)$ for barium are now known to be as follows :

Barium.

Series $\nu=(1\cdot5, S)-(m, P)$.

$m=$	2	3	4	5	6	7	8	9	10
λ(Å.U.) =	5535	3275	2845	2597	2542	2498	2470	2455	2441
$\nu=$	10864·6	30534·4	35149·4	38502·1	39339·1	40032	40486	40733·2	40966·8

The limiting frequency $\nu=(1\cdot5, S)$ for the series comes out by calculation to be 42006·85, and the corresponding shortest wave length of the series is given by $\lambda=2380\cdot56$ Å.U. This leads to an evaluation of the ionisation potential for barium, which has been deduced to be 5·21 volts. In passing, it may be noted that the discovery of the series $\nu=(1\cdot5, S)-(m, P)$ for barium has enabled us to calculate the wave lengths of the two series $\nu=(2\cdot5, S)-(m, P)$ and $\nu=(3\cdot5, S)-(m, P)$ as well for the same element. It has also led us to predict that the wave length corresponding to the frequency $\nu=(1\cdot5, S)-(2, p_2)$ is approximately $\lambda=7901\cdot11$ Å.U.

The recent experiments of Tate and Foote * point the way for a direct determination of the ionisation potentials of the vapours of magnesium, calcium, strontium and barium, and it will be interesting to see whether the experimental values so obtained for these vapours agree with the values calculable for them from the limiting frequencies of the singlet series $\nu=(1\cdot5, S)-(m, P)$ of their spectra. The ionisation potentials for the different elements, in so far as they appear to be known have been collated and are given in Table II. Table III. also contains the ionisation potentials for a number of simple and compound gases and vapours which have been determined experimentally by Hughes and Dixon.

(V.) *Low-voltage Arcs in Metallic Vapours.*

In a Paper by Millikan† and in one by Hebb‡ experiments are described in which arcs were established in mercury vapour bombarded by electrons when the P.D.s applied were

* Tate and Foote, " Phil. Mag.," July, 1918, p. 64.
† Millikan, " Phys. Rev.," Vol. IX., No. 5, p. 378, 1917.
‡ Hebb, " Phys. Rev.," Vol. IX., No. 5, p. 371, 1917.

TABLE II.—*Ionisation Potentials.*

Element.	Values determined by direct experiment.*	Calculated values.†
Helium	20·5 volts.	29·3 volts.
Neon	16·0 ,,	—
Argon	12·0 ,,	—
Hydrogen	11·0 ,,	—
Oxygen	9·0 ,,	—
Nitrogen	7·9 ,,	—
Mercury	10·4 ,,	10·45 volts.
Zinc	9·5 ,,	9·4 ,,
Cadmium	8·92 ,,	9·0 ,,
Magnesium	—	7·65 ,,
Calcium	—	6·12 ,,
Strontium	—	5·70 ,,
Barium	—	5·21 ,,
Sodium	5·13 volts	5·13 ,,
Potassium	4·1 ,,	4·32 ,,

* Aston, "Proc." Roy. Soc., June 14, 1907; Frank and Hertz, "Ber." d Deut. Phys. Ges., Heft 2, p. 44, 1913; Bazzoni, "Phil. Mag.," Nov., 1916; Tate, "Phys. Rev.," Vol. X., No. 1, p. 81, 1917; Bergen Davis and Goucher, "Phys. Rev.," Vol. X., No. 2, p. 101, 1917 Bishop, "Phys. Rev.," Vol. X., No. 3, p. 244, 1917; Wood and Okano, "Phil. Mag.," Sept., 1917, p. 177; Tate and Foote, "Phil. Mag.," p. 64, July, 1918.

† Bohr, "Phil. Mag.," Vol. XXVIII., Feb., 1915; McLennan and Henderson, "Proc." Roy. Soc A, Vol. XCI., p. 485, 1915; McLennan, "Proc." Roy. Soc. A, Vol. XCII., p. 305, 1916; McLennan, "Phys. Rev.," Vol. X., No. 1, p. 84, 1917: Richardson and Bazzoni, "Phil. Mag.," Oct., 1917, p. 285; Tate and Foote, "Phil. Mag.," p. 64, July, 1918.

TABLE III.—*Ionisation Potentials by Hughes and Dixon.**

Vapour or gas.	Ionisation potential.
H_2	10·2 volts.
O_2	9·2 ,,
N_2	7·7 ,,
S	8·3 ,,
Cl_2	8·2 ,,
Br_2	10·0 ,,
Hg	10·2 ,,
HCl_1	9·5 ,,
CO	7·2 ,,
CO_2	10·0 ,,
NO	9·3 ,,
CH_4	9·5 ,,
C_2H_6	10·0 ,,
C_2H_4	9·9 ,,
C_2H_2	9·9 ,,

* Hughes and Dixon, "Phys. Rev.," Vol. X., No. 5, p. 495, 1917.

considerably less than those which the experiments of Bergen Davis and Goucher have indicated are requisite to accomplish the ionisation of mercury atoms.

In particular, Millikan and Hebb found that under certain specified experimental conditions it was possible to cause arcs to strike in mercury vapour with applied P.D.s as low as 4·7 volts. Moreover, they also found that arcs so struck could be maintained in the vapour when the applied field was reduced to as low as 3·2 volts. Hebb also found that it was possible under certain circumstances to cause the vapour to radiate monochromatic light of wave length $\lambda = 2536{\cdot}72$ Å.U. when the applied field in which the electrons were projected was as low as 2·5 volts. As these results would appear to be in conflict with what should be expected on the quantum theory, namely, that the ionisation potential is given by $V = \frac{(1{\cdot}5, S).h}{e}$, where $(1{\cdot}5, S)$ is the frequency of the shortest wave length of the principal singlet series $\nu = (1{\cdot}5, S) - (m, P)$, it was considered necessary that the experiments should be repeated and the results either confirmed or disproved. With a view to doing this some experiments were recently made for me by Mr. R. Hamer and Mr. F. W. Kemp * in the Physical Laboratory at Toronto. In these the vapours of cadmium and zinc, as well as that of mercury, were used, and the results obtained go to confirm the findings of Millikan and Hebb. The main results of the investigation are as follows :—

1. It has been shown that increasing the temperature of the incandescent cathode lowers the voltage necessary to produce arcs in the vapours of mercury, zinc and cadmium.

2. With limed platinum cathodes arcing voltages were not obtained as low as with incandescent tungsten filaments.

3. With highly heated incandescent cathodes it was found that with vapour of low density the arcing potentials were high. When the density of the vapour was increased the arcing potentials fell, and reached a minimum value at a certain density of the vapour. When the density of the vapour was still further increased the values of the arcing potentials rose again.

4. The voltages necessary to maintain arcs in the vapours were found to be less than those which had to be applied in order to strike the arcs.

* McLennan. Communicated to The Physical Society of London, Oct., 1918.

5. With mercury it was found possible to strike arcs with arcing potentials as low as 4·75 volts, and to maintain them with an applied P.D. of 2·84 volts. With cadmium vapour arcs were struck with impact voltages of 5 volts, and maintained with P.D.s of 2 volts. To obtain these low-voltage arcs it was necessary to use intensely hot cathodes and a copious supply of highly heated metallic vapour.

6. In many of the experiments it was noticed that when certain voltages near the minimum ones were applied, no arc appeared in the vapours forthwith, but arcs did finally strike when an interval of time of greater or shorter length was allowed to elapse.

7. With moderately heated incandescent cathodes and a moderate supply of metallic vapour the arcing voltages with mercury, zinc, and cadmium vapours under electronic bombardment were given by the quantum relation. $V=\frac{(1{\cdot}5, S)\times h}{e}$, where $(1{\cdot}5, S)$ is the frequency of the shortest wave length in the $\nu=(1{\cdot}5, S)-(m, P)$ series.

8. In the experiments with cadmium vapour it was found to be possible to set up two types of arc in the vapour, the one being faint and the other brilliant. The line spectrum of the two types appeared to be the same in character, but different in intensity. Additions of P.D. in the applied field of not more than a volt generally sufficed to enable one to pass from the faint to the brilliant type. With the brilliant arcs a continuous white light spectrum was superimposed upon the line spectrum.

9. Irregularities in the experimental results were in a measure removed when the metal being vaporised, was kept electrically connected to the positive terminal of the discharge tube.

(VI.) *Discussion of Results.*

From the results of Bergen Davis and Goucher, combined with those of other investigators, it would appear to be generally agreed that with the vapours of such elements as mercury, zinc, cadmium and magnesium, for example, the actual ionisation potentials are given by $V=\frac{(1{\cdot}5, S)\times h}{e}$, where $(1{\cdot}5, S)$ is the frequency of the shortest wave length of the principal singlet series $V=(1{\cdot}5, S)-(m, P)$. The

ionisation potentials for the metals indicated are respectively 10·45 volts, 9·4 volts, 9 volts and 7·65 volts. It is also certain that when arcs are established in these vapours the atoms of the vapour of these elements are ionised. The experiments of Millikan, Hebb, Hamer and Kemp also clearly show that with the vapours of mercury and cadmium it is possible to strike arcs by electronic bombardment with incandescent cathodes when the applied impact voltages are as low as 4·75 volts with mercury and 5 volts with cadmium.

It is also evident that with mercury vapour arcs can be maintained with impact voltages as low as 2·84 volts, and with cadmium vapour with impact voltages as low as 2 volts. In the case of argon, Mackay and Fergusson * have also shown that with an incandescent tungsten cathode it is possible when the gas is carefully purified to maintain an arc with potential drops a little less than 4 volts, and that with the gas at a pressure of 5 cm. of mercury. This potential drop in the gas, it will be noted, is considerably less than the ionisation potential for argon, which is given by Frank and Hertz † as 12 volts.

The question then to be settled is, whence comes the energy requisite to produce ionisation in the case of these low-impact voltage stimulated arcs ?

Among others, two explanations have been put forward the one by Millikan and Hebb, ‡ involving the principle of "photo-electric radiation reaction," and the other by Van der Bijl, § involving the principle of "successive impacts."

According to the first mentioned explanation, we are to assume that with impact voltages of about 5 volts the kinetic energy of the bombarding electrons is sufficient to stimulate the vapour to the emission of the monochromatic radiation of frequency $\nu=(1{\cdot}5, S)-(2, p_2)$, which for mercury is $\lambda=2536{\cdot}72$ Å.U. and for cadmium is $\lambda=3260{\cdot}17$ Å.U. This monochromatic radiation is then supposed to react upon the incandescent cathode, and possibly, too, upon condensed layers of the vapour which may be present on the walls of the tube in the neighbourhood of the cathode, and so bring about the emission of electrons with initial speeds greater than those which the electrons would have from thermionic considerations alone. These highly speeded electrons would then,

* Mackay and Ferguson, "Journ." of the Franklin Institute, p. 209, Feb., 1916.

† Frank and Hertz, "Ber." d. Deutsch. Phys. Ges., XV., p. 34, 1914.

‡ Millikan and Hebb, loc. cit.

§ Van der Bijl, "Phys. Rev.," Vol. XI., No. 3, p. 170, March, 1918.

after passing through the applied electric field, be able to stimulate radiation of still shorter wave lengths than that of frequency $\nu=(1\cdot5,\ S)-(2,\ p_2)$, and this would in turn react upon the incandescent cathode and again cause the emission of electrons with still higher initial speeds. The result of the application of this principle, if it is a valid one, would be that in any case of radiation stimulated by electronic bombardment whenever impact voltages were applied, which would suffice to stimulate the radiation of frequency $\nu=(1\cdot5,\ S)-(2,\ p_2)$ all the conditions would be existent for the production of the many lined spectrum. There could be on this theory no such thing as a vapour under any conditions of density and temperature emitting a radiation consisting of one wave length and one wave length only. Again, according to the principle of " successive impacts " invoked by Van der Bijl, we are to suppose that the same atom can be successively bombarded by different electrons, and that as a result of these successive bombardments the atom ultimately becomes ionised. Van der Bijl adheres to the Bohr theory of the origin of radiation and supposes that although each bombarding electron may not possess sufficient kinetic energy to ionise an atom by itself, still, at each impact, the bombarding electron may be able to impart sufficient energy to the atom struck to cause one or more electrons of its system to be projected into a stationary atomic orbit with a larger radius than the one it was in before the atom struck. The potential energy of a bombarded atom would thus increase by stages under multiple impacts, and this would go on until ionisation occurred.

With the atoms ionised it would follow that the vapour was in the condition to emit radiation corresponding to the many lined spectrum. It will be seen, therefore, that the hypothesis of the principle of " successive impacts " also leads to the view that the radiation from a vapour when bombarded by electrons, be the density of the vapour high or low, cannot be monochromatic, but must be such as to give the many lined spectrum.

Another view to take of the matter is to suppose that the distribution of velocities among the electrons emitted by an intensely hot tungsten filament is such that a considerable number of these have initial velocities corresponding to potential falls of from 5 to 6 volts. If this be so it is not necessary to assume either the hypothesis of " radiation

reaction " or that of " successive impacts," for with applied fields of about 5 volts, the high-speed electrons mentioned above will, in the applied field, acquire sufficient kinetic energy to ionise the vapour. This view also leads, it will be seen, to the result that a vapour bombarded by such streams of electrons must emit a radiation which can not be monochromatic, but must be one which will produce the many lined spectrum.

As the distribution of velocities indicated above will depend only upon the temperature of the tungsten filamental cathode, it follows that even with very low densities the vapour bombarded must emit a panchromatic radiation.

All three hypotheses, therefore, lead to the same result, namely, that it is impossible to stimulate a vapour by electronic bombardment to the emission of a radiation of one particular wave length. This result would appear then to be in direct contradiction to the experimental results of McLennan and Henderson, McLennan and Ireton, Bergen Davis and Goucher, Tate and others who found that with vapour bombarded by streams of electrons of moderate density it was possible by using suitable impact voltages to obtain radiation from the vapour which registered on the photographic plates, even with hours of exposure, only a single wave length. The contradiction may turn out, however, to be only apparent, for it may well be that while the average or mean speed of the electrons bombarding the vapour may suffice to bring out a radiation of one wave length with comparatively strong intensity the number of electrons possessing velocities higher than the mean falls off so rapidly with the higher speeds that, under the ordinary conditions which obtain, the amount of energy associated with the radiations of wave length shorter than the first one is too excessively small to be detected photo-electrically or photographically. The effects of absorption also come in, and add to the difficulty of obtaining indications of the presence of the shorter wave lengths. This view would fit in both with the results of Bergen Davis and Goucher with mercury vapour, as well as with those of McLennan and Ireton with zinc and cadmium vapours, in which it was found that with certain applied voltages given by the quantum relation it was possible to register an emission of radiation of one wave length only from the vapour bombarded and by gradually increasing the impact voltages to bring out a second radiation of a shorter

wave length which again corresponded to the quantum relation. To account for this result it is only necessary to suppose that by far the greater number of electrons leave the incandescent cathode with zero initial velocities. All the recent experimental work on the production of characteristic radiations from metals bombarded in a vacuum by electrons points to the accuracy of this view, for it has been definitely shown that a given characteristic radiation is given out only when the impact voltages of the electrons reach the value given by the quantum relation for this particular radiation. Richardson's theory of the thermionic emission of electrons makes it abundantly clear that while the great majority of the electrons may leave an incandescent filament with very small velocities still certain percentages will be emitted with velocities ranging practically to infinity. The fractions possessing the higher velocities are, however, necessarily small. Various estimates have been formed of the average speed of emission of the electrons from an incandescent filament. Among others, Richardson and Brown * place the average velocity of emission of electrons from an incandescent platinum filament at 0·6 volt, and from a limed platinum filament at 1·2 volts. Wood and Okano † give 0·4 volt as the average velocity with which electrons leave an incandescent tungsten wire, but Tate and Foote ‡ point out that one explanation of their results requires the average velocity of emission to be 1·6 volts. Both Wood and Okano and Tate and Foote have expressed the view that an appreciable fraction of the electrons are emitted with velocities still higher than the equivalent of 1·6 volts potential drop. Hebb § suggests 2 to 3 volts as possibly representing under certain circumstances the velocity of electronic emission. It seems legitimate then to assume that while the most important of the effects produced by the electronic bombardment of a vapour may demand a zero initial velocity for the electronic emission from an incandescent cathode still it is conceivable that other effects of a less pronounced character may be brought into evidence under special conditions which indicate that an appreciable fraction of the electrons emitted from a highly heated incandescent tungsten filament may leave it with very considerable initial

* Richardson and Brown, "Phil. Mag.," Vol. XVI., 1908.
† Wood and Okano, "Phil. Mag.," Sept., 1917.
‡ Tate and Foote, "Phil. Mag.," July, 1918.
§ Hebb, "Phys. Rev.," May, 1917.

velocities. Take, for example, the case of mercury and cadmium vapours stimulated under electronic bombardment to the emission of a radiation giving their many lined spectra when impact fields as low as 5 volts were used. Here it is only necessary to assume that small but appreciable fractions of the bombarding electrons possess initial thermionic velocities as high as the equivalent of about 5 volts. That this is conceivable would seem to be warranted by the evidence adduced above.

For the stimulation of a vapour to the emission of panchromatic radiation by electronic bombardment with low impact voltages without the production of so-called arcs in the vapour, the requisite conditions would appear to embrace the use of vapours of comparatively high density and the use of intensely dense streams of electrons from an extremely hot incandescent cathode. These, it will be recalled, are exactly the conditions which applied in the experiments o. Kemp, in which he obtained with low fields the many lined spectrum from cadmium vapour without any trace of the presence in the background of a white light spectrum, and with a marked absence of the great brilliancy which usually characterises the line spectrum of an arc.

The argument which has been developed above leads, it will be seen, to the view that the emission from a vapour bombarded by electrons of a radiation consisting of the wave lengths which make up its line spectrum does not necessarily connote the simultaneous existence of a so-called arc in the vapour. As actual arcs can be struck in metallic vapours bombarded by electrons with low-impact voltage fields it is necessary, however, in order to explain their production, to invoke the agency of factors other than the streams of electrons which owe their existence to thermionic emission from the incandescent cathode. These latter, it will be seen, suffice on the view presented to produce, in conjunction with low fields, ionisation in the vapour and the consequent emission of a radiation giving the many lined spectrum, but for the production of arcs as they are ordinarily observed, we must look in another direction for an additional supply of electrons. A possible source of such a supply is afforded by the presence in the vapour of positive ions formed by the primary thermionically emitted electrons when these are accelerated in the applied field.

With an applied field of about 5 volts it would be impossible to give these positive ions sufficient kinetic energy to ionise the atoms of mercury vapour, for example, assuming the ionisation potential of the latter to be 10·4 volts. It is conceivable, however, that they could acquire sufficient kinetic energy in the electric field to produce when impinging upon the incandescent cathode an emission of electrons from the latter with initial velocities so high that on passing through the electric field they acquired additional kinetic energy sufficient to bring about ionisation of the vapour. If the quantum relation applied to this hypothetical emission of electrons from the cathode under bombardment by the positive ions the highest mean initial kinetic energy that the emitted electrons could have would be that represented by a potential fall of about 5 volts. Consequently, the highest kinetic energy that these electrons could acquire in passing through the electric field would be that corresponding to about 10 volts, which is practically the ionisation potential of mercury atoms. If, then, the positive ions are to be considered as playing a main rôle in the production of arcs in vapours bombarded by thermionically emitted electrons it is evident that one could not hope to strike arcs in these vapours with applied impact voltages smaller in magnitude than one-half the value of the ionisation potentials of these vapours. As the ionisation potentials for mercury vapour and for cadmium vapour are probably 10·4 volts and 9·4 volts respectively, and as it was found that the lowest impact voltages with which arcs could be struck in these vapours were respectively 4·75 and 5 volts, this result would seem to indicate that the positive ions do play an important part, in the manner indicated, in bringing about the establishment of the arcs. The explanation of the fact that arcs when once struck in a vapour can be maintained with lower applied fields than the minimum ones which are requisite to cause the arcs to strike, is not altogether clear. This phenomenon, however, is not confined to vapours alone, since it is known to characterise arc discharges in gases generally. Factors such as thermionic emission from the hot gases or vapours, thermal molecular agitation, and possibly chemical action as well, may have a bearing on the matter. It is to be remembered, too, that when once an arc has been established in a vapour or in a gas, the molecules are no longer in their normal state. Consequently, the considerations which apply in dealing with

the question of the maintenance of arcs may be quite different from those which apply in an explanation of their initial production.

(VII.) *General Results.*

Among the general results which appear to emerge as unassailable from the investigations dealt with in the present communication is the view that the ionisation potential is a definite and determinate magnitude, varying with and characterising each particular type of atom. It would also appear that, while this magnitude may be determined experimentally for each element, provided the requisite physical conditions can be realised, it can also probably be deduced by the quantum theory from a knowledge of the value of the frequency $\nu=(1\cdot5, S)$ of the shortest wave length in the principal singlet series $\nu=(1\cdot5, S)-(m, P)$ of the spectrum of the element. It would also appear to be established that it is possible to cause a vapour bombarded in a vacuum by electrons to emit a radiation consisting of one wave length and one wave length only, provided *all* the bombarding electrons possess kinetic energy given by the quantum relation $Ve=h\nu$, ν being the frequency of the monochromatic radiation stimulated; moreover, it would appear that by gradually increasing the speed of the bombarding electrons the vapour may be caused to emit at successive stages radiation of shorter and shorter wave lengths, each particular wave length being stimulated only when the bombarding electrons have attained velocities corresponding to its frequency as indicated by the quantum relation. All apparent departures from this view obtained in any experiments would appear to be explainable when account is taken of the fact that a Maxwellian distribution of velocities holds for streams of electrons thermionically emitted by incandescent metallic filaments.

(VIII.) *Infra red Stimulation and Two Type Ionisation Potentials.*

The inquiry into the origin of radiation described above has recently been given as impetus in another direction by some results obtained in the infra red region of the spectrum by one of my students, Mr. R. C. Dearle,* at Toronto. In 1913 attention was specially drawn in a Paper by McLennan and

* Dearle ," Roy. Soc.," A., Vol. XCII., p. 608, 1915.

Dearle * to the importance of the wave length $\lambda=10140$ Å.U. in the spectrum of mercury. This wave length, which is just four times the wave length of $\lambda=2536 \cdot 72$ Å.U., is the first member of the series $\nu=(2 \cdot 5, S)-(m, P)$, and possesses the greatest amount of energy of all the wave lengths in the radiation emitted by the mercury arc. It has been shown by Dearle, moreover, to be readily absorbed by ordinary non-luminous mercury vapour even when the density of the latter is very low. Quite recently Dearle † has also shown that mercury vapour bombarded by electrons can be made to emit radiation of wave-length $\lambda=10140$ Å.U. when impact voltages as low as 5 volts are used. The quantum relation requires an impact voltage of 1·26 volts for the stimulation of this wave length, and it would be interesting to see if the vapour can be made to emit radiation of wave length $\lambda=10140$ by using an impact voltage of this amount.

The investigation is, however, one of great difficulty. In order to get over troubles arising from absorption, the vapour pressure of the mercury must be very low, and when vapours of low density are used the intensity of the radiation emitted is consequently feeble. With linear thermopiles of the most sensitive type and a Paschen galvanometer, such as Dearle used, it is difficult to make definite and satisfactory measurements when the radiation investigated becomes very feeble in intensity. As far as Dearle's experiments take us, however, they lend some support to the view that the quantum relation will give the requisite voltage for the stimulation from mercury vapour of an emission of the radiation $\lambda=10140$ Å.U., just as it has been found to apply to the stimulation of radiation consisting of single line, double line or many line spectra from the vapours of mercury, zinc, cadmium and magnesium under the experimental conditions already specified. If it should turn out to be true that mercury vapour in its ordinary condition can be made to emit the radiation $\lambda=10140$ Å.U., under bombardment by electrons possessing kinetic energy corresponding a P.D. of 1·26 volts, there is suggested the possibility of stimulating the vapour, by increasing the impact voltages, to the emission of radiation consisting of the wave lengths constituting the higher members of the series $\nu=(2 \cdot 5, S)-(m, P)$, of which $\lambda=10140$ Å.U. is the first

* McLennan and Dearle. "Phil. Mag.," Vol. XXX., p. 683, 1915.
† Dearle. Communicated to The Royal Society, Oct., 1918.

member. This, in turn, suggests the possibility of there being a new type of ionisation potential for mercury atoms corresponding to the frequency $\nu=(2\cdot5,\ S)$. On evaluating this ionisation potential by the quantum relation, it comes out as approximately 2·5 volts, which is just about one-quarter of the accepted value of the ordinary type of ionisation potential for mercury. What bearing Dearle's results may have on the views presented in this Paper is not as yet clear, but it is evident that they open up a line of attack for still further extending our knowledge of the origin of radiation, and possibly, too, of adding to what we know at present of atomic structure.

I.—*Low-voltage Arcs in Metallic Vapours.* By *Prof.* J. C. McLennan, *F.R.S.*

Received October 21, 1918.

I. Introduction.

In a Paper by Millikan* and in one by Hebb† experiments are described in which arcs were established in mercury vapour bombarded by electrons when the P.D.s applied were considerably less than those which the experiments of Bergen Davis and Goucher‡ have indicated are requisite to accomplish the ionisation of mercury atoms.

In particular Millikan and Hebb found that under certain specified experimental conditions it was possible to cause arcs to be struck in mercury vapour with applied P.D.s as low as 4·7 volts. Moreover, they also found that arcs so struck could be maintained in the vapour when the applied field was reduced to as low as 3·2 volts. Hebb also found that it was possible, under certain conditions, to cause the mercury vapour to radiate out light of wave length $\lambda=2536{\cdot}72$ Å.U., when the applied field in which the electrons were projected was as low as 2·5 volts. As these results would appear to be in conflict with what should be expected on the basis of the quantum theory, namely, that the ionisation potential is given by $V=\frac{(1{\cdot}5,\ S)h}{e}$, where $(1{\cdot}5,\ S)$ is the frequency of the shortest wave length in the principal singlet series $\nu=(1{\cdot}5,\ S)-(m,\ P)$, it was considered necessary that the experiments should be repeated and the results either confirmed or disproved.

With a view to doing this, some experiments were recently made for me by Mr. R. Harmer and Mr. F. W. Kemp in the Physical Laboratory at Toronto.

In these the vapours of cadmium and zinc, as well as that of mercury, were used, and the results obtained go to confirm the findings of Millikan and of Hebb. An account of the experiments follows.

* Millikan, "Phys. Rev.," Vol. IX., No. 5, p. 378, 1917.
† Hebb, "Phys. Rev.," Vol. IX., No. 5, p. 371, 1917.
‡ Bergen Davis and Goucher, "Phys. Rev.," Vol. X., No. 2, 1917.

II. Experiments by R. Harmer.

(a) *Discharge Tubes.*

In the course of these experiments it was found that the potentials necessary for producing arcs in mercury vapour varied capriciously and irregularly with changes in the temperature of the incandescent cathodes with changes in the density and temperature of the mercury and with changes in other factors not specially under control. It was decided to clear up these difficulties, if possible, and various modifications were made in the arrangement and form of the different parts of the discharge tube.

The form of tube which ultimately gave regular uniform and concordant results is that shown in Fig. 1.

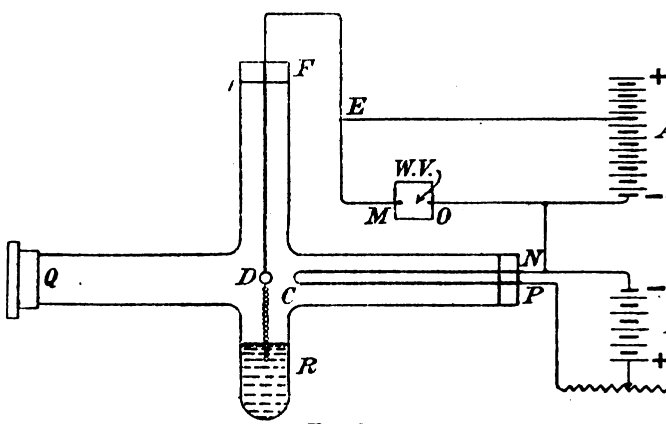


Fig. 1.

The tube itself was made of fused quartz. *C* was an incandescent cathode and *B* the heating battery. *PC* and *NC* were leading-in wires. *A* was the impact voltage battery. The positive terminal *EF* was an insulated iron wire terminated by an iron ring *D*, whose plane was at right angles to the axis of the discharge tube. A short chain with iron links kept the ring in electrical connection with the mercury in the reservoir of the discharge tube. This latter arrangement, it was found, contributed very definitely to obtaining regularity in the results. As shown in Fig. 1, *N* and *P* were respectively the negative and positive terminals of the heating battery. The negative terminal of the "impact voltage" battery was connected to *N*, and the positive terminal to *E*.

Readings on a standard Weston voltmeter W.V. joined up between M and N, gave values which were taken as the "impact voltage" between the ring S and the cathode C. With the arrangement shown it will be seen that these readings represented the maximum impact voltages present in the tube, including the potential fall in the incandescent cathode itself. The leading-in wires E, F, NC and PC, were stout, and had practically a negligible resistance.

(b) Density and Temperature of the Mercury Vapour.

With the incandescent cathode, whether simply a tungsten wire or a limed platinum strip kept with a definite and fixed heating current passing through it, it was found that as the density of the vapour increased, and possibly also the temperature, the applied voltages necessary to establish arcs gradually fell.

In one particular set of experiments in which a limed platinum strip cathode was used the reservoir R was about half filled with mercury. The cathode was then made incandescent, and the mercury was gently heated with the hot gases from a Bunsen burner placed near R. The potential required to produce an arc under these circumstances was 17·6 volts. The Bunsen burner was then placed so that its flame was directly in contact with R, and the mercury was made to boil. Under these circumstances the arcing potential fell to 10·2 volts.

The burner was then removed and the mercury allowed to cool. After about 1½ minutes a series of arcing potentials was begun, and readings were taken at intervals. This series, which began at 14·2 volts, showed that as the mercury cooled the P.D. necessary to produce arcs gradually rose to as high as 23·8 volts. With the last-mentioned voltage the mercury in R was quite cool. The record of results is given in Table I.

These results, which are typical of many others which were obtained, show that the density of the vapour, and possibly also the temperature of it, had much to do with the lowering of the arcing potential. As the tungsten filament was intensely bright in the experiments III., and as the reservoir of mercury was some 6 cm. or 7 cm. below the filament, it is not considered that the heat from the flame affected to any appreciable extent the temperature of the filament. It should also be noted that no fluorescence of the mercury vapour was visually observed during the heating of the mercury.

TABLE I.

I. Platinum-limed Cathode.

State of mercury.	Arcing potential in volts.
Mercury warmed with hot gases	17·6
Mercury hotter	17·0
Flame in contact with reservoir	10·27
	10·26
	10·26
	10·26
Mercury left to cool for 1¼ minutes	14·2
Mercury gradually cooling	14·9
	16·3
	17·0
	17·9
	18·8
	19·3
	19·6
	20·6
	21·4
	22·0
	22·6
Mercury quite cool to hard	23·8

II. Tungsten Filament.

Heating current.	Arc struck at
14 amperes	7·6 volts.
,,	9·4 ,,
,,	9·67 ,,
,,	9·92 ,,
,,	10·2 ,,
,,	10·44 ,,
,,	10·6 ,,
,,	10·64 ,,

REMARKS.—Mercury at first made very hot, and Bunsen burner was then removed, and mercury allowed to cool gradually.

III. Tungsten Filament Cathode Carrying 14·4 Amperes.

State of mercury.	Arcing potential in volts.
Mercury hot, flame low, no bubbling	8·8
Mercury hotter, flame higher, mercury still not bubbling	7·2
Mercury much hotter, and bubbling	6·2

(*c*) *Results.*

The series of results given in (*b*) shows that the arcing potentials which were obtained when a tungsten filament was used were much lower than those obtained with a platinum cathode. This result was invariably obtained. It may have been due to the fact that it was not found possible to heat the platinum to as high a temperature as the tungsten. This would probably result in the volume and average speed of the streams of electrons from the cathode being greater with the tungsten filaments than they were when the platinum strips

were used. In all the experiments, unless specially mentioned otherwise, tungsten wire 15 mils in diameter was used. In the case of platinum the cathode was made of a narrow strip of thin sheet metal.

(*d*) *Temperature of the Incandescent Cathode.*

In all the experiments it was found that whether the mercury vapour was dense or rare, the arcing potentials always decreased as the temperature of the cathode was raised. This again may have been due to a consequent increase in the volume and average speed of the streams of electrons ejected. The following examples serve to illustrate the point :—

TABLE II.

I. Tungsten Filament.

State of mercury.	Arcing potential in volts.	Heating current, amps.	Remarks.
Mercury quite hot and surface continually agitated	5·7	14·0	Filament very brilliant.
Ditto	5·4	13·9	Filament brilliant.
Ditto	5·2	13·2	Filament not quite so brilliant.
Ditto	6·15	12·25	Filament bright.
Ditto	6·6	11·1	Filament red.
Ditto	7·5	10·1	Filament still red.
Ditto	No arc even at 23·4 volts	8·5	Filament dull red.

II. Tungsten Filament.

State of mercury.	Arcing potential in volts.	Heating current. amps.	Remarks.
Mercury warm, but surface not agitated	10·8	14·0	Filament very bright and glowing.
Ditto	13·6	11·0	Filament incandescent.
Ditto	20·0	9·7	Filament bright.
Ditto	None at 24·0	9·2	Filament red.

III. Tungsten Filament.

State of mercury.	Arcing potential in volts.	Heating current, amps.	Remarks.
Mercury hot and surface agitated	6·2	14·4	Filament very bright.
Mercury very hot and surface extremely agitated.	5·5	17·2	Filament extremely bright.

(e) Arcing Voltages and Potentials Requisite to Maintain Arcs.

In all the experiments it was observed that the voltage required to make the arc strike was generally higher than the P.D. necessary to maintain the arc. The establishment of the arc for a given applied voltage was always immediately accompanied by a drop of greater or lesser magnitude in the potential between the two electrodes of the discharge tube. When this occurred it was possible to lower the potential between the electrodes still further without the arc being extinguished. In these regards the phenomena of the arc were exactly the same as those which are known to characterise electric discharge in gases at low pressures produced by applying a gradually increasing P.D. to the terminals of a discharge tube. The following are illustrations of this effect :—

TABLE III.

I. Tungsten Filament.

Mercury quite hot and agitated.

Heating current.	Arc struck at	Arc extinguished at
Amps.	Volts.	Volts.
14·0	5·7	3·64
13·9	5·4	3·4
13·2	5·2	3·4
12·25	6·15	3·7
11·1	6·6	5·0
10·1	7·5	7·2

II. Platinum Limed Cathode.

Heating current.	Arc struck at	Arc extinguished at	Remarks.
Amps.	Volts.	Volts.	
7·3	17·6	14·6	Mercury hot, but not agitated.
,,	10·26	9·0	Mercury very hot.
,,	14·2	13·8	Mercury cooling.
,,	17·9	17·5	,, ,,
,,	21·4	20·8	,, ,,
,,	22·0	21·5	,, ,,
,,	22·6	21·8	,, ,,
	23·8	22·6	,, ,,

III. Tungsten Filament.

Heating current.	Arc struck at	Arc extinguished at	Remarks.
Amps.	Volts.	Volts.	
9·3	11	9·0	Filament bright, mercury very hot.
9·1	18·1	17·1	Filament less bright, mercury cooler.
14·4	8·8	4·5	Mercury hot, no bubbling
14·4	7·2	4·2	Mercury hot, surface not agitated.
14·4	6·2	3·75	Mercury very hot and bubbling.
17·5	5·5	4·7	Mercury extremely hot.

IV. Tungsten Filament.

Heating current.	Arc struck at	Arc extinguished at	Remarks.
Amps.	Volts.	Volts.	Filament glowing.
14	7·6	6·5	Mercury strongly heated
	9·4	8·7	at first, and then al-
	9·67	9·35	lowed to cool slowly.
	9·92	9·74	
	10·2	9·94	
	10·44	10·23	
	10·6	10·4	
	10·64	10·45	

(*f*) *Minimum Arcing Potentials.*

From the results given above it is clear that the conditions which determined the establishment of the arc are somewhat complex. The general effect, however, of some of the main factors involved is evident. High temperature filaments and highly heated mercury were the factors which led to the lowest arcing voltages.

In a special experiment in which the cathode was a 10 mil tungsten wire heated almost to the melting point a series of results was obtained with the mercury strongly heated. These gave the lowest arcing potentials observed. In recording the readings sufficient time was allowed to elapse before taking each set in order to make certain that the vapour was under normal conditions. The readings are given in Table IV.

TABLE IV.

Arc struck at	Arc extinguished at
4·9 volts	3·4 volts.
4·96 "	3·25 "
4·75 "	2·86 "
4·95 "	2·84 "
5·0 "	2·87 "
5·01 "	3·25 "

From these results it will be seen that the lowest voltage at which it was found possible to strike the arc in mercury was 4·75 volts. The lowest P.D. which sufficed to maintain such an arc when once struck was 2·84 volts. These results are in good agreement with those obtained by Hebb, who found, as stated above, that under certain conditions arcs in mercury vapour could be struck with impact voltages of 4·7 volts, and maintained with applied P.D.s down to 3·2 volts.

It should be pointed out that in all the experiments described the distance between the edge of the positive ring electrode and the incandescent filament was about 1·5 cm.

Throughout the experiments small variations of 2 mm. or 3 mm. in this distance did not appear to change the arcing voltages. It would have been interesting to see if variations in the electrode distance of a somewhat greater amount produced any effect on the results. This point, however, was not specifically examined.

III. Experiments by F. W. Kemp.

I. *Arcing Potentials in Zinc and Cadmium Vapours.*

(*a*) *General Conditions.*

In the first of these experiments the conditions governing the establishment of arcs in zinc and in cadmium vapour were studied. The form of discharge tube used is shown in Fig. 2, and was the same as that used by McLennan and Henderson.* The streams of bombarding electrons were of moderate intensity. Three quartz tubes were fused to a central receptacle *C* to form a **T**. At *A* and *E* the electrodes *B* and *D* were introduced through ebonite plugs, sealed in the ends of the tube with wax. At the end of the third branch a ground joint *G* was fitted with a glass tube. Into the glass tube were sealed two stout low-resistance wires, *H* and *K*, leading to the filament *F*. *F*, *B* and *D* each just reached to the edge of

* McLennan and Henderson, "Proc." Roy. Soc., A., Vol. XCI., p. 485.

the receptacle C, in which was placed the metal to be studied. Through the ebonite plug at E a tube was connected to an air pump. During all the experiments the apparatus was kept highly exhausted. K and D were externally connected, thus F and D constituted a double cathode with B as the anode. B and D were made from rods of the metal whose vapour was being studied. The filament F was heated by an alternating current, and the metal in C was melted with a gas burner. A direct current P.D. was applied between B and D.

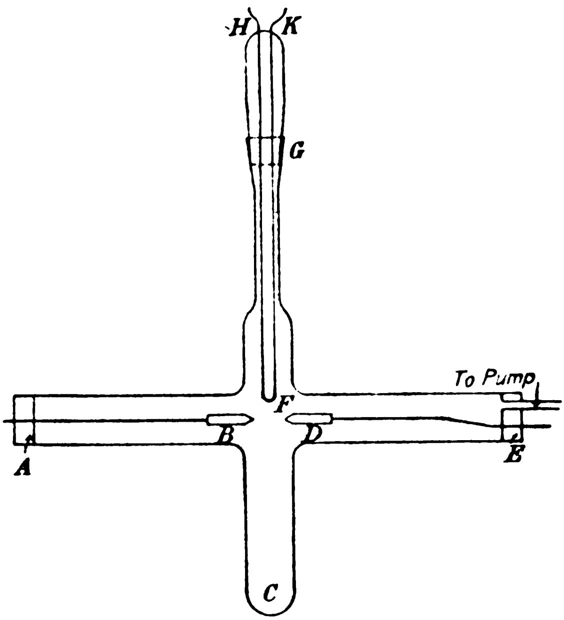


FIG. 2.

With the filament, which was a 10 mil tungsten wire at a bright red or white heat and the metal boiling, it was found that the arc in both zinc and cadmium vapours would strike at between 10 and 12 volts. It would persist to a lower voltage, though with certain densities the striking and breaking voltages were more nearly equal than with others. With the vapour either very dense or very rare the difference was large. The least difference seemed to come with densities corresponding to a temperature slightly below the boiling point of the metal. This density also gave the lowest values for the

arcing potential. If the flame were removed from the receptacle C the arcing potential lowered a little as the metal cooled and then went up quite rapidly.

Also, as the supply of metal in the receptacle became low, the arcing potential went up. Below are some values taken consecutively just as the last few drops of molten metal boiled away:

TABLE V.

Zinc.			Cadmium.		
Current in filament.		Arcing potential	Current in filament.		Arcing potential.
Amps.	Volts.	Volts.	Amps.	Volts.	Volts.
9·1	3·0	10·7	9·25	3·3	10·3
		16·0	9·1	3·3	10·8
		16·8			11·0

It could not be stated definitely whether this rise was due to a variation in the temperature or in the density of the vapour, as the form of apparatus did not lend itself to the separation of these factors. It was probably, however, a density effect.

In both zinc and cadmium vapours two types of arc were observed. At a certain applied voltage the arc spectrum could just be seen in the spectroscope. By increasing the voltage it became brighter, but when a certain voltage was reached there was a very abrupt increase in brilliance, though no new lines were observed. This brilliant arc was never observed except with very dense vapour. With zinc this jump in brilliance occurred at a potential 0·7 to 1 volt higher than the lowest applied voltage at which the arc could be seen. With cadmium the difference was 0·5 to 1·2 volts. If the vapour were not so dense the brilliant arc could not be produced with 25 volts.

(*b*) *Effect of Rise in Temperature of Cathode.*

The temperature of the tungsten wire cathode was a large factor in altering the magnitude of the arcing potential. If this wire were not white hot no brilliant arc was obtained. The minimum arcing potential decreased as heavier currents were passed through the filament. This variation could be observed more closely than that due to vapour density or temperature. The two accompanying curves, Figs. 3 and 4, illustrate the effect of changing the temperature of the hot

cathode. The minimum arcing potential was plotted against the current through the filament cathode, which may be taken to represent approximately the temperature of the latter. The values plotted in Figs. 3 and 4 are given in Table VI.

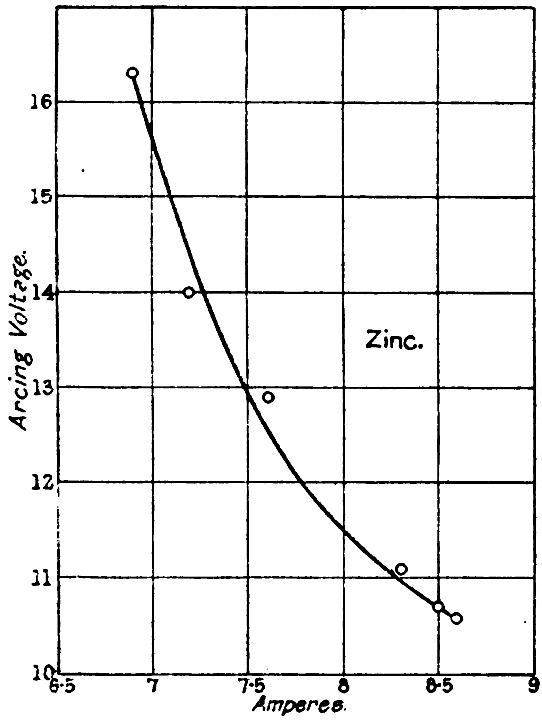


FIG. 3.

TABLE VI.

Zinc.		Cadmium.	
Current through filament.	Minimum arcing potential.	Current through filament.	Minimum arcing potential.
Amps.	Volts.	Amps.	Volts.
6·75	16·3	7·15	15·9
7·1	14·0	7·4	14·2
7·6	12·9	7·65	13·1
8·25	11·1	8·0	12·1
8·5	10·7	7·9	11·6
8·6	10·6	8·2	11·3
		8·15	11·2
		8·6	10·7
		8·6	10·4

The effect of variation in the temperature of the filament was shown in another way. A certain potential was applied which was not sufficient to bring on the arc. The temperature of the

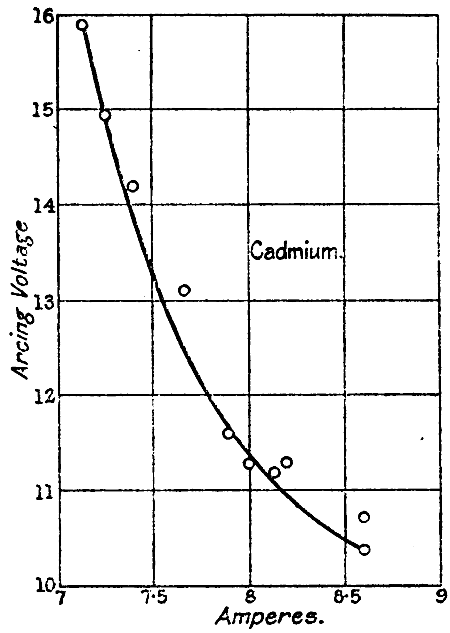


FIG. 4.

cathode was then raised, and the heating current noted as the arc became visible. This, however, lacked the precision that accompanied the former method, but as illustrative of the phenomenon the results are given :—

TABLE VII.

Zinc.		Cadmium.	
Fixed applied impact voltage.	Minimum current in filament when arc struck.	Fixed applied impact voltage.	Min. current in filament when arc struck.
Volts.	Amps.	Volts.	Amps.
10·2	9·3	10·1	8·5
10·3	9·3	10·4	8·25
11·1	9·1	10·6	7·95
10·4	8·2	11·6	7·9
11·2	7·6	12·0	7·8

As seen from Table VII., the minimum arcing potential was different under different conditions. There were, however, values below which with this type of apparatus the arcs would not strike. These values approximated very closely to the values calculated from the quantum hypothesis, though experiments to be described later will show that these were not the lowest values of the arcing potentials obtainable. The table of values (Table VIII.) were the minima of several sets of readings. The size of the wire used for the hot cathode was the same for each, but not the length. There was also a slight variation in vapour density and distance between electrodes for the different values.

TABLE VIII.—*Minimum Arcing Potentials.*
First Set.

Zinc.		Cadmium.	
Calculated from $V=\frac{h(1\cdot5,\ S)}{e}$	Experiment.	Calculated from $V=\frac{h(1\cdot5,\ S)}{e}$	Experiment.
Volts.	Volts.	Volts.	Volts.
9·24	10·49	8·85	10·3
	10·0		10·1
	9·7		10·06
	9·5		9·6
	9·4		9·3

In obtaining these values a standard voltmeter gave the P.D.s between B and D, and a second one the P.D.s between H and K. As the heating current was an alternating one the values given in Table VIII. were obtained by adding the direct current potential fall between F and B to the maximum value of the P.D. in the alternating-current cycle between H and K.

The results of these experiments, it will be seen, are in practical agreement with the results of the original experiments of McLennan and Henderson,* where it was found that the impact voltages which brought on the arc and consequently the many lined spectrum, were given by the quantum relation $Ve=h\nu$, ν being the frequency of the shortest wave length of the series of wave lengths given by $\nu=(1\cdot5,\ S)-(m,\ P)$. Two points of special importance which were brought out in these experiments are (1) that the minimum arcing potential corresponded to a definite density of the vapour bombarded, and (2) that there were two types of arc

* McLennan and Henderson, loc. cit.

obtainable in vapours of considerable density. With both types the many lined spectrum of the metal vaporised was obtained, but the two types differed markedly in brilliancy, and it was possible to pass from the faint type to the brilliant type by a small addition to the impact voltage applied. A point which should be noted in regard to this matter is that with the brilliant type of arc there was always present and superimposed upon the line spectrum a strong continuous white light spectrum. With the faint type of arc, while the line spectrum stood out clearly, there was a total absence of anything like a continuous white light spectrum in the background.

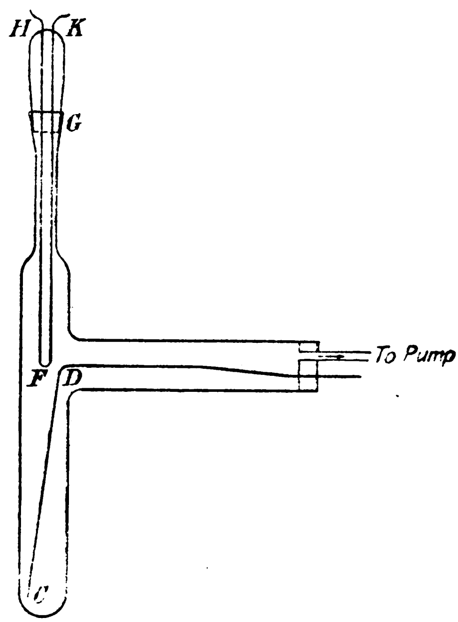


FIG. 5.

II. *Arcing Potentials in Mercury Vapour.*

(*a*) *General Conditions.*

Some experiments were also carried out with mercury vapour. For these the tube shown in Fig. 5 was used. In some of the experiments tungsten wire 10 mil in diameter was used for the hot cathode, and in others platinum foil carrying a spot of calcium oxide was used. The tube, Fig. 5, differed from that in Fig. 2, in that there was a single cathode, namely, the hot filament *F*. *D*, the anode, was made of steel

wire dipping down into the receptacle C, which contained the metal to be vaporised. The tube was made of fused quartz, and the metal in C was vaporised by the heat trom a Bunsen burner.

Mercury.

The arcing potential for mercury vapour was found to depend upon the temperature of the cathode, much as was the case with zinc and cadmium. The mercury was always kept boiling, and the potential at which the arc broke was lower by ½ volt than the striking potential. In this regard, too, mercury vapour behaved similarly to the vapours of cadmium and zinc.

With the tungsten wire kept intensely hot and the mercury boiling the arc was obtained with impact voltages as low as 6 volts. With platinum foil bearing a spot of calcium oxide and heated to a white heat a P.D. as low as 8 volts established the arc. The platinum foil would not stand the same high temperature as the tungsten filament, and the difference in the arcing voltage was probably due to a cause associated with this condition.

(*b*) *Experiments with Hot Cathodes and Anodes.*

In a special set of experiments a tube was arranged with the electrodes similar to the negative electrode in Fig. 5. Each electrode had its own heating circuit, and the impact voltages were applied by means of a battery inserted between the circuits of the two electrodes. In these experiments the arc in mercury vapour was never obtained with less than 12 volts. The temperature of the anode had little effect, if any, and, as the values given below indicate, the heating of the anode did not in any way produce a lowering of the arcing potential. The following are the results obtained in one set of observations:—

TABLE IX.

Heating currents.		Arcing potential.
Cathode.	Anode.	
Amps.	Amps.	Volts.
6·2	4·7	12·0
6·2	5·7	12·3
6·2	7·4	13·6

The form of apparatus used in these experiments helped to bring out the effect of variations in vapour density and

temperature. With the mercury boiling the minimum arcing potential was found. Then the flame was removed, and as the apparatus cooled the minimum value diminished for a short time, but as further cooling took place it rose rapidly. Table X. illustrates this effect, V_1 representing the potential required, while the metal was boiling, and V_2 the arcing potential at the turning point as the vapour cooled.

TABLE X.

Heating currents.		Arcing potential.	
Cathode.	Anode.	V_1.	V_2.
Amps.	Amps.	Volts.	Volts.
5·8	4·7	18	14
6·2	4·7	14	12
6·2	5·7	13	12

III. *Low-voltage Arcs in Cadmium Vapour.*

In working with the cadmium vapour a third set of experiments was made with a tube of the form shown in Fig. 6.

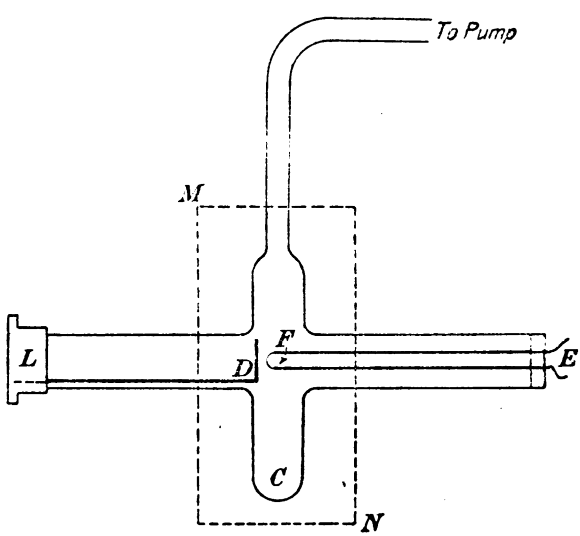


FIG. 6.

This tube had three branches of fused quartz, one of which was used only to connect it to the pump. The cathode *F* was a 15 mil tungsten wire, the leads entering through the ebonite plug at *E*. The anode *D* was a heavy iron wire

ring, whose centre was in line with the cathode, and whose edge was everywhere not more than 4 mm. from the latter. Over the end of the tube there was waxed an iron sleeve, L, carrying a glass window, and the wire leading to the anode was soldered into this sleeve. The receptacle C and the neighbouring parts were heated by an electric furnace, $M\ N$, and the temperature of the metallic vapour in C was calculated from the temperature coefficient of resistance of the wire composing the furnace. The filament F was heated by a direct current, the negative end of which was connected to earth. The arcing potential was ascertained by measuring the P.D. between D and the earthed lead of the filament.

With the use of an electric furnace it was possible to maintain the vapour in the discharge tube much more definitely at any selected temperature than it was with the use of a Bunsen burner. In the present instance the furnace was always maintained at a temperature of 400°C.

With the apparatus in this form the arc was found to have all the characteristics previously described. The difference in the voltage required to cause the arc to strike and that required to maintain it was quite pronounced. It was also found that it was possible to cause arcs to strike with voltages considerably below those demanded by the quantum theory on the basis of the ionisation potential being given by $V=\frac{h\times(1{\cdot}5,\ S)}{e}$, $(1{\cdot}5,\ S)$ being the frequency of the shortest wave length of the series $\nu=(1{\cdot}5,\ S)-(m,\ P)$.

The results given in Table XI. will serve to make these points clear.

TABLE XI.—*Arcs in Cadmium Vapour.*

The arc struck at	P.D. between electrodes dropped to	The arc broke at	P.D. between electrodes rose to
Volts.	Volts.	Volts.	Volts.
7·2	4·2	3·2	4·4
6·9	4·0	3·1	3·9
6·6	3·7	2·8	2·8
6·4	3·4	3·1	3·8

From the numbers given in this Table it will be seen that when arcs were struck with a given applied voltage the current in the arcs caused a drop in the actual P.D. between the electrodes. Moreover, when arcs were once established, it was necessary, as the numbers show, to reduce the voltage

applied externally in order to extinguish them. The drop in voltage between the electrodes just before the arcs were extinguished, it will be seen, was a little below the actual drop between the electrodes just after extinction had taken place.

As the ionising potential for cadmium given by the quantum theory is 8·85 volts, it will be seen that for cadmium vapour as for mercury vapour it is possible under certain conditions of cathode temperature and vapour pressure to establish arcs with applied P.D.s considerably below the value of the ionisation potential.

With the cathode at an intensely high temperature the arc would strike easily between 6 and 7 volts. In one particular case the applied P.D. was set at 5·5 volts, and the arc struck in 5 seconds. When set at 5·0 volts the arc struck after 2 minutes waiting. In this case when the applied potential was lowered the arc still persisted at 2 volts. At this low voltage the difference in potential between the positive end of the cathode filament and the anode was only of the order of 0·1 volt. When the applied P.D. was set slightly lower than 5 volts no arc appeared even with a 10-minute wait. In all the experiments the arc was never observed when the applied P.D. was lower than 2 volts. These low readings were taken when the cathode was extremely hot, and only a few readings could be taken before the filament had to be renewed through fusing.

Summary of Results of the Experiments by Harmer and Kemp.

1. It has been shown that increasing the temperature of the incandescent cathode lowers the voltage necessary to produce arcs in the vapours of mercury, zinc and cadmium.

2. With limed platinum cathodes arcing voltages were not obtained as low as with incandescent tungsten filaments.

3. With highly heated incandescent cathodes it was found that with vapour of low density the arcing potentials were generally high. When the density of the vapour was gradually increased the arcing potential fell and reached a minimum value at a certain density of the vapour. When the density of the vapour was still further increased the values of the arcing potentials rose again.

4. The voltages necessary to maintain the arcs in the vapours were found to be less than those which had to be applied in order to strike the arcs.

5. With mercury it was found possible to strike arcs with arcing potentials as low as 4·75 volts, and to maintain them an applied P.D. of 2·84 volts sufficed.

6. With cadmium vapour arcs were struck with impact voltages of 5 volts, and maintained with P.D.s of 2 volts. To obtain these low arcing voltages it was necessary to use intensely hot cathodes and a copious supply of highly heated metallic vapour.

In many of the experiments it was noticed that when certain voltages near the minimum ones were applied no arc appeared in the vapour forthwith, but arcs did finally strike when an interval of time of greater or shorter length was allowed to elapse.

7. With moderately heated incandescent cathodes and a moderate supply of metallic vapour the arcing voltages with mercury, zinc and cadmium vapours under electric bombardment were given by the quantum relation $V=\frac{h\times(1{\cdot}5,\ S)}{e}$, where (1·5, S) is the frequency of the shortest wave length in the $\nu=(1{\cdot}5,\ S)-(m,\ P)$ series.

8. In experiments with cadmium vapour it was found to be possible to set up two types of arc in the vapour, the one being faint and the other brilliant. The line spectrum of the two types appeared to be the same in character, but different in intensity. Additions of a P.D. in the applied field of not more than a volt generally sufficed to enable one to pass from the faint to the brilliant type. With the brilliant arcs a continuous white light spectrum was superimposed upon the line spectrum.

9. Irregularities in the experimental results were in a measure removed when the metal being vaporised was electrically connected to the positive terminal of the discharge tube.

The Physical Laboratory, University of Toronto,
July 1, 1917.

DISCUSSION ON

"THE CASE FOR A RING ELECTRON."

A Meeting of the Society was held on Friday, October 25, 1918, at the Imperial College of Science, South Kensington, when a discussion took place on "The Case for a Ring Electron."

The chair was taken by the President, Prof. C. H. LEES, F.R.S., who called upon Dr. H. S. Allen to open the discussion.

II. *The Case for a Ring Electron.* *By* DR. H. S. ALLEN.

FOR many purposes it is sufficiently accurate to regard the unit of negative electricity as a point charge, but for a more exact determination of its properties it is necessary to assign some definite shape to the distribution of electricity constituting the electron. Thus, the shape is often assumed to be that of a sphere or spheroid. Although the spheroidal form possesses advantages from the standpoint of mathematical analysis, there are many reasons why it is preferable to assume that the electron is in the form of a current circuit which can produce magnetic effects. Then the electron, in addition to exerting electrostatic forces, behaves like a small magnet. In its simplest form the magnetic electron may be looked upon as a circular anchor ring of negative electricity which rotates about its axis with a velocity which is certainly large, and is perhaps comparable with that of light. Parson* has suggested that the name *magneton* should be applied to this electron; but, as this term has already been employed in a somewhat different sense, I prefer to speak of the ring electron.

With reference to the corpuscular theory of light, Sir Isaac Newton wrote: "I shall not assume this or any other hypothesis, not thinking it necessary . . . yet, while I am describing this to avoid circumlocution and to represent it more conveniently, I shall speak of it as if I assumed it and proposed it to be believed." In collecting the arguments in favour of the ring electron I do not wish it to be understood that I am

* Parson, "Smithsonian Misc. Coll.," Vol. LXV., No. 11, 1915.

in agreement with them all or with the views of all those who have supported this hypothesis. Some of the arguments have been well stated in Parson's Paper on a magneton theory of the structure of the atom.

Part I.—The Arguments for a Ring Electron.

1. *Loss of Energy by Radiation.*—The most important difficulty in connection with the classical electron is that of the radiation which must take place, according to the laws of electrodynamics, when the electron (or a number of electrons) is circulating in an orbit. This difficulty, though perfectly well known, is usually ignored altogether in the discussion of atomic theories. When the number of electrons in the orbit is increased the loss of energy is greatly diminished, and the difficulty may be met to a certain extent by postulating rings containing many corpuscles. We now know, however, that in the case of the lighter atoms—hydrogen, helium, lithium—the number of electrons is small; in fact, it appears certain that Moseley's atomic numbers, $H=1$, $He=2$, $Li=3$ . . . must be accepted as representing the number of electrons in the atom. The only way left of meeting the difficulty for the classical electron is to postulate such a "binding" of the electron to the nucleus as precludes radiation in the stationary state of the system (Bohr). This is, of course, purely hypothetical, and it is certainly simpler to do away with the difficulty altogether by assuming the rotation of an annular change. Radiation will then occur only in the case of disturbances or irregularities in the annular motion.

2. *Diamagnetism.*—The second objection to the prevailing theory arises in connection with the explanation of diamagnetic atoms—and most substances are diamagnetic. In order to account for a zero resultant magnetic moment, the independent orbits must be considered to have their axes uniformly distributed in three dimensions. Interference between separate rings of this sort would result in an altogether chaotic motion in the atom. Whilst such chaotic motion would be consistent with diamagnetism, it would invalidate all those considerations which presuppose the existence of definite periods of vibration in the atom. The question is discussed at some length in Parson's Paper. The substitution of the ring electron for Langevin's electron in orbital motion removes the fundamental difficulties of his theory, but leaves the

superstructure almost intact. That this substitution can be made will be made clear by a short quotation from the conclusion of his Paper :—

" . . . and we can form a simple and exact picture of all the facts of magnetism and of diamagnetism by imagining the individual currents produced by the electrons to be indeformable but movable circuits of no resistance and very great self-induction, to which all the ordinary laws of induction are applicable."

3. *Paramagnetism.*—In the older theory there are certain outstanding difficulties in connection with the explanation of paramagnetic properties. No satisfactory explanation has yet been given as to how the orbits become tilted under the influence of an external magnetic field. Although it may not be easy to develop a complete mathematical treatment of the behaviour of the ring electron in a magnetic field, it would certainly seem easier to understand the tilting of such a system than that of a single particle moving in a closed orbit.

By means of X-ray photographs showing the Laue diffraction pattern for crystals of magnetite, hematite and pyrrhotite in the magnetised and unmagnetised state, K. T. Compton and Trousdale* came to the conclusion that the atoms do not leave their positions of equilibrium during magnetisation. These results may be regarded as consistent with any form of the electron theory of magnetism, but the hypothesis of ring electrons seems to afford a particularly clear and straightforward explanation of the phenomenon.

A further step in the analysis was taken by A. H. Compton and O. Rognley†, who examined the intensity of the beam of X-rays reflected from a crystal face. In no case was any change observed in the intensity of the reflected beam when the crystal was magnetised or demagnetised, though the method was sufficiently sensitive to detect a variation in the intensity of less than 1 per cent. Since the intensity depends upon the arrangement of the electrons in the atoms which make up the crystal the authors conclude that it is neither a group of atoms, such as the chemical molecule, nor the atom itself which is the elementary magnet. We must look to the atomic nucleus, or to the electron for the ultimate magnetic particle.

* K. T. Compton and Trousdale, "Phys Rev.," Vol. V., p. 315, 1915.
† A. H. Compton and O. Rognley, "Phys. Rev.," Vol. XI., p. 132, 1918.

The existence of a zero point energy of rotation is supported by the magnetic susceptibility of paramagnetic substances. According to Oosterhuis* most of the deviations from Curie's law found at low temperatures may be explained quantitatively on this assumption. This view has been supported by Keesom.† It receives a natural explanation if the ring electron be accepted, for the electron would retain its magnetic properties at low temperatures.

The existence of a mechanical effect accompanying magnetisation was foretold by Richardson‡ in 1908. Assuming that the resultant magnetic fields which the atoms of magnetic (as opposed to diamagnetic) substances possess, arise from the motion of their constituent electrons in closed orbits, there must be a moment of momentum per unit volume proportional to the intensity of magnetisation. Einstein and De Haas§ succeeded in observing the effect predicted by Richardson, and obtained a factor of proportionality corresponding to that which would be due to negative electrons. Experiments at Princeton by Stewart|| led to a value only one-half that obtained previously. The magnitude of this momentum can be accounted for if positive, as well as negative, charges are moving within the atom, but in opposite directions. A similar result was obtained in the work of S. J. Barnett,¶ who showed that a rotating cylinder of iron becomes magnetised. This may be regarded as the converse of the Richardson effect. The relation of these phenomena to the properties of the ring electron has been discussed by Webster.**

4. *The Asymmetry of Certain Types of Radiation.*—In order to explain the asymmetry of scattered X-radiation A. H. Compton†† has examined theoretically the scattering to be expected when the electron is in the form of a spherical shell, each part of which can scatter independently and may be capable of rotational motion. He found that it was then possible to explain not only the asymmetry of the scattered rays, but also the diminution of scattering with decrease of wave length. Since the mass of an electron cannot be

* Oosterhuis, "Phys. Zeitschr.," Vol. XIV., p. 862, 1913.
† "Konink. Akad. Amsterdam," Vol. XVI., pp. 454 and 468, 1913.
‡ Richardson, "Phys. Rev.," Vol. XXVI., p. 248, 1908.
§ Einstein and De Haas, "Deutsch. Phys. Gesell.," Vol. XVII., p. 152, 1915.
|| Stewart, "Phys. Rev.," Vol. XI., p. 100, 1918.
¶ S. J. Barnett, "Phys. Rev.," Vol. VI., p. 239, 1915.
** Webster, "Phys. Rev.," Vol. IX., p. 484, 1917.
†† Compton, "Journ." Wash. Acad. Sci., January 4, 1918.

accounted for on the basis of a uniform distribution of electricity over the surface of a sphere, Compton suggested that the true shape of the electron may be that of a ring. His estimate of the radius is $2{\cdot}3 \times 10^{-10}$ cm., but reasons have been given for supposing that this estimate must be reduced to about one-tenth of the value stated.*

The same kind of asymmetry occurs in the case of corpuscular radiation excited when X-rays fall on a thin plate of any material. E. A. Owen† has recently carried out experiments which show that no difference can be detected between the ratio of emergent to incident corpuscular radiation when the screen from which the corpuscles are ejected, is changed from the crystalline to the amorphous state. This result renders doubtful the explanation of asymmetry put forward by H. A. Wilson, who attributed it to the difference between the behaviour of crystalline and amorphous material, and suggests that an explanation of a more fundamental character is necessary to account for this phenomenon. This view is supported by the fact that the ratio of emergent to incident corpuscular radiation is approximately the same for the two salts investigated and for the metals, gold and silver. It would appear that the asymmetry must be due to some property of the electron itself and not of the atom from which the electron is liberated.

5. *Absorption of X-rays by Magnetic Substances.*—It has been observed by A. H. Forman‡ that iron has a slightly greater absorption coefficient for X-rays when magnetised parallel to the transmitted beam than when unmagnetised.§ This may be attributed to the fact that when the axis of the ring electron is parallel with the incident X-rays the energy scattered by the electron is a maximum. (A. H. Compton.)

6. *Ionisation of Gases by X-rays.*—It has long been known that the ionisation produced in a gas by Röntgen radiation or ultra-violet light is remarkably small; when the ionisation is strong the ratio of the free ions to the number of gas molecules is less than $1 : 10^{12}$. This may be explained by some form of the unitary theory of light, but an alternative explanation

* "Nature," Vol. C., p. 510, 1918.

† E. A. Owen, "Proc." Phys. Soc., Vol. XXX., p. 133, 1918.

‡ A. H. Forman, "Phys. Rev.," Vol. VII., p. 119, 1916.

§ In Faraday's note-book is found the suggestive query: "Does this [magnetic] force tend to make iron and oxide of iron transparent?"

may be suggested to the effect that there may be only one plane in which the electrons can absorb sufficient energy from the Röntgen ray for ionisation.*

These ideas may have some bearing upon the work of Righi† on the ionisation of gases by X-rays in a magnetic field. Experiments carried out under very varied conditions led to the conclusion that the magnetic field tends to favour ionisation of a gas by diminishing the energy required. Thus an icn or an electron in motion can ionise a gaseous atom on collision, when a magnetic field exists, although the kinetic energy of the electron does not reach the minimum value that is necessary in the absence of the magnetic field. This effect is termed "magnetc-ionisation."

7. *Thermo-Electric Effects.*—Grondahl‡ claims to have obtained experimental evidence for the existence of an electron endowed with a magnetic moment. Such an electron would be affected by a non-uniform magnetic field. A conductor placed in such a field would therefore gain a negative potential in that part which lies in the stronger portion of the field. A magnetic field should produce an effect on the thermoelectric force of magnetic substances. This effect was examined in the case of a copper iron couple, the iron member of which was a short wire which could be placed in a magnetising coil. Theory predicts an increase in the thermal E.M.F., and this was actually observed and found to be of the order of magnitude to be expected.

8. *The Radiation Formula of Planck.*—A method of deducing Planck's radiation formula by making certain assumptions as to the internal mechanism of Parson's "magneton" has been given by Webster.§ Energy is stored in the rotating ring in a non-radiating form. "If the electricity is movable on the ring in any other way than as a rigid mass, the alternating external force of a light wave will induce oscillations on it capable of absorption and radiation of energy. These induced oscillations were shown to give an explanation of refraction, diffraction and allied phenomena almost exactly like that of the classical

* Cf. A. H. Forman, *loc. cit.*
† Righi, "R. Accad. Sci.," Bologna, Vol. IV., p. 1, 1916-1917; Vol. V., p. 3, 1917-1918.
‡ Grondahl, "Phys. Rev.," Vol. X., p. 586, 1917.
§ Webster, Amer. Acad. "Proc.," Vol. L., p. 131, 1915; "Phys. Rev.," Vol. VIII., p. 66, 1916.

electron theory. At the same time the internal mechanism assumed above for transferring energy between the oscillations and the circulation can be supposed to damp the induced oscillations, transferring their energy to the rotation, until the energy accumulated above the initial value reaches some definite multiple of the quantum. At such a point it is assumed that the process may be reversed, starting a large oscillation, which will be maintained constant at the expense of the circulation, until the excess energy is all radiated away. The probability, η, that such an oscillation will start when the stored energy reaches a given multiple of $h\nu$ is given by Planck's condition $(1-\eta)/\eta=pI$, where I is the mean square of the electric field per unit frequency interval, and p is determined so as to give the Jeans-Rayleigh law at low frequencies. The entropy of the system is then found by Planck's equation $S=k \log W$, where the microscopic state of a system is determined by the total accumulated energy of each oscillator. The derivation of Planck's law in this theory is therefore almost exactly like his."

Attention may again be drawn to the significance of Planck's constant, h, which may be regarded as a quantum of action. The view, first suggested by Nicholson, that h represents an angular momentum is simpler and easier to realise. McLaren identified the natural unit of angular momentum with the angular momentum of the magneton, and it has been pointed out several times that this implies proportionality to the magnetic moment of the magneton. Thus the ring electron serves to give an intelligible meaning for this "universal" constant, and at the same time suggests the possibility of a relation between h and e. Such a relation has in fact been put forward by Lewis and Adams in the form

$$15c^3h^3=8\pi^5(4\pi e)^6,$$

an equation based on a certain assumption as to the form of the constant in Stefan's law of radiation.

9. *Series of Lines in Spectra.*—The theory of line spectra proposed by Bohr has met with a considerable amount of success at least in connection with the hydrogen spectrum, and the developments made by Sommerfeld and others have afforded an explanation of the fine-structure of the hydrogen lines. It may prove possible to restate Bohr's theory and, whilst retaining its essential features, modify it so as to apply

it to the case of the ring electron. Thus it might be suggested that the "stationary state" of the electron in its orbit corresponds to a particular value of the radius of the annular electron, the change from one stationary state to another corresponding to a definite change in the radius. It would, of course, be necessary to introduce some hypothesis to fix the size of the ring, just as it is necessary to postulate the stationary state in Bohr's theory. In the last-named theory no detailed assumptions are made as to the mechanism of transition between two stationary states or as to the mode of emission of radiation. In the case of the ring electron radiation might arise from pulsations of the ring in its passage from one stationary condition to another. In this connection it is interesting to recall the views of Sutherland * with regard to the origin of lines in spectral series. He came to the conclusion that the series must arise from kinematical considerations and explained them by considering the nodal sub-divisions of a circle.

It may be well again to draw attention to the fact that, as shown by Nicholson, coplanar rings of electrons are not possible when electrostatic forces alone are taken into consideration. Provided the electrons are in one plane they must form a single ring. The assumption of rings of electrons is no longer required if both electron and core are endowed with magnetic properties, as the electrons may then be supposed to take up positions of statical equilibrium with reference to the core—a condition of affairs which is in harmony with the facts of chemical combination and crystalline formation.

10. *The Zeeman Effect.*—In the simple theoretical explanation of the Zeeman effect first given by Lorentz, the motions of the electrons in a luminous source are analysed into three components, a vibration parallel to the lines of magnetic force and two circular motions, clockwise or anticlockwise, in planes normal to the lines of force. The vibrations are assumed to take place under the influence of "elastic" forces, attracting the electrons towards a position of equilibrium. Although explanations of the magnetic effect have been given by the combination of the quantum hypothesis with Bohr's atomic model, Debye has emphasised the fact that with these assumptions there is no place left for the quasi-elastic oscillating electrons which have been used in

* Sutherland, "Phil. Mag.," Vol. II., p. 245, 1901.

all theories for the explanation of the Zeeman effect from Lorentz to Voigt. Mention should be made of the theory of Ritz in which the electron moves in an orbit in a plane perpendicular to the axis of a magnet formed by the addition of a number of elementary magnets, which are the same for all substances.

If radiation is due to pulsations in a ring electron, it would seem that the Zeeman effect should follow from reasoning similar to that first employed by Lorentz.

For consider a "pulse" in the form of a small "hump" travelling round the ring. This may be regarded as equivalent to a particle of mass km and charge ke (where k is a fraction of the total mass or total charge) travelling in a circular path under the influence of elastic forces. A magnetic field would produce a change of period (positive or negative) as in the theory of Lorentz proportional to ke/km, that is to e/m.

The complex resolutions sometimes observed under the influence of the magnetic field may possibly be explained as arising from more complicated pulsations of the ring. Runge has made known a rule which states that the complicated magnetic resolutions are in simple relation with the normal value of e/m. Ritz has endeavoured to explain the magnetic resolutions by a kind of precessional movement of the system round the lines of force as axis. This theory and those put forward by Lorentz and Voigt may be found discussed in the last chapter of Zeeman's "Researches in Magneto-Optics." "Voigt has abandoned the supposition of magnetically isotropic, arbitrarily orientated particles, and modified Lorentz's system of equations.. He supposes that the radiating particles are orientated under the action of the field." The results obtained on this assumption are competent to explain all the resolutions observed. The hypothesis of the ring electron appears to furnish exactly what is required for a basis of this theory. In particular it explains the difficulty as to the absence of an undisplaced line in light emitted perpendicular to the field, for the electron would set at right angles to the magnetic field and there would be no motion of parts of the ring parallel to the lines of magnetic force.

11. *Solar Electron Streams.*—The view that magnetic storms and the auroræ are due to ionisation of the outer regions of the atmosphere by streams of electrons or ions

projected from the sun is now commonly received. Schuster * has drawn attention to the fact that the electrostatic forces due to a cloud of charged particles would tend to scatter the particles to a considerable extent. If, however, the electron possesses the properties of a small magnet, a little consideration will show that the magnetic forces would tend to diminish the scattering effect. This may make it easier to understand the sudden commencement of magnetic storms, which would be difficult to explain if the electron stream is widely scattered before reaching the earth. Even if the effect referred to is small, it may not be negligible in the case of a crowd of electrons moving in a magnetic field.

12. *Chemical Considerations.*—Serious difficulties are met with when an attempt is made to apply the conception of the electron at present in vogue to problems of chemical constitution and stereochemistry. Any theory in which the electrons are in rapid orbital motion is difficult to reconcile with the stereochemical evidence for a definite spatial arrangement of the groups of atoms attached to a carbon atom, especially as the rings of electrons must usually all rotate about the same axis. These difficulties have been discussed at length by Parson. What seems to be required is the existence of chemical " linkages " or " bonds " having definite relations in space with reference to the atom, and yet admitting of a certain mobility. This involves the presence of valence electrons which are at rest, or vibrating within narrow limits, near the surface of the atom. Stark has developed a theory on these lines, taking into consideration only electrostatic forces. Nicholson † has discussed the stability of such systems and has shown that Stark's conclusions do not survive a quantitative treatment. It appears to be impossible for two atoms in a molecule to be linked by a single electron, or by two electrons, which attract both atoms.

The introduction of a magnetic electron, producing both an electric and a magnetic field, furnishes a means of avoiding these difficulties, so that it becomes possible to retain stationary electrons and at the same time the orbital motion which is required to explain radiation and magnetism. It is perhaps well to emphasise the fact that this theory includes *both electrostatic and magnetic* attractions and repulsions. It

* Schuster, " Proc." Roy. Soc., Vol. LXXXV., p. 44, 1911.
† " Proc." Phys. Soc., Vol. XXX., p. 65, 1918.

is probable that the electrostatic forces play the most important part in the formation of chemical compounds, especially in those of " polar " type, whilst the magnetic forces may be regarded as permitting a stable arrangement of stationary atoms and electrons.

The presence of eight groups in the periodic table indicates that for the majority of elements eight electrons are required to form a stable system. This is brought out in the arrangement of the table which I published recently in the " Transactions " of the Chemical Society.* In place of Newland's " law of octaves " we have what may be termed the " rule of eight." This rule is brought into prominence when atomic numbers are inserted in the periodic classification.

The same rule is obeyed in the case of chemical compounds, as is shown most clearly by the " molecular numbers " which I have introduced. The molecular number signifies the sum of the positive charges carried by the atomic nuclei contained in the molecule. Both Parson and Lewis † have emphasised the fact that the groupings required by chemistry are, in general, of two sorts, one a pair of electrons closely associated, and the other a very compact group of eight. These are exactly the groupings that might be expected from magnetic doublets, for Parson has shown that the group of eight forms an extremely stable system of low magnetic energy.

13. *Cohesion.*—That the cohesion of a solid arises from the action of electric or electro-magnetic forces may be inferred from optical experiments. The Lorentz-Fitzgerald hypothesis explains the negative result of the Michelson-Morley experiment by a contraction of the material framework of the apparatus in the direction of its motion through the æther. Such a contraction, which is essential according to the principle of relativity, may be predicted from the standpoint of electromagnetic theory. There is, therefore, a strong presumption that the forces of cohesion between the particles, which give a solid its rigidity are either electric or magnetic forces.

All chemical forces also are probably of electromagnetic origin, and in the light of our present knowledge it does not seem possible or advisable to distinguish sharply between chemical and physical forces. Indeed, Langmuir,‡ in an interesting Paper on the constitution and fundamental pro-

* H. S. Allen, Chem. Soc. "Trans.," Vol. CXIII., p. 389, 1918.
† Lewis, " Journ." Am. Chem. Soc., Vol. XXXVIII., p. 762, 1916.
‡ Langmuir, " Journ." Amer. Chem. Soc., Vol. XXXVIII., p. 2221, 1916.

perties of solids and liquids, concludes that both solids and liquids consist of atoms held together entirely by *chemical forces*. The phraseology may serve a useful purpose if it serve to emphasise the essential identity between the forces imparting rigidity to a crystal and those conferring stability on the molecule of an organic compound.

In this connection it is of interest to recall the work of Lewis * on the relation between the internal pressure or cohesion P in the equation

$$(p+P)(v-b)=RT,$$

and the dielectric capacity and magnetic permeability of a liquid. He has shown that the Obach Walden relation regarding the proportionality between the internal pressure and the dielectric constant follows from the hypothesis that molecular attraction is electromagnetic, not electrostatic in nature.

Part II.—The Properties of the Ring Electron.

The Mass of the Electron.

The electron was originally regarded as a sphere or spheroid, and in this case the electromagnetic mass may be calculated as was first shown by J. J. Thomson. The electromagnetic inertia for the slowly moving sphere, charge e, radius a, is $2e^2/3ac^2$, where c is the velocity of light.† For the Lorentz electron, for which the shape alters as the acceleration proceeds, the longitudinal and transverse masses are equal to

$$2e^2/3ac^2(1-k^2)^{3/2} \text{ and } 2e^2/3ac^2(1-k^2)^{1/2}$$

(where k denotes the ratio of the speed of the centre to that of light), as required by the Principle of Relativity.‡

The question of the electromagnetic mass of the ring electron has been discussed by Webster,§ who has shown that the relativity principle requires certain assumptions about the internal energy, which, when made in the most plausible way, lead to the result that the mass of the electron is $2/c^2$ times its electrostatic energy. In ordinary units the mass is given by

$$M=\frac{e^2}{\pi c^2 R}\log\frac{8R}{R'},$$

* W. C. McC. Lewis, "Phil. Mag.," Vol. XXVIII., p. 104, 1914.
† *See* Schott, "Electromagnetic Radiation," Appendices C and D.
‡ Schott, "Proc." Roy. Soc., Vol. XCIV., p. 422, 1918.
§ Webster, "Phys. Rev.," Vol. IX., p. 484, 1917.

where R is the radius of the ring, and R' is the radius of the cross-section of the ring. This makes the radius of cross-section extremely small in comparison with the radius of the ring.

The same assumptions lead to the conclusion that the gyroscopic properties of the ring electron are exactly those of a classical electron in an orbit having the same magnetic moment.

The Magnetic Moment of the Ring Electron.

For simplicity we may suppose that the electron is composed of a charge e distributed round a circle of radius a, and moving with angular velocity ω. Then the moment of the equivalent simple magnet is $\frac{1}{2}ea^2\omega$. If m denote the electromagnetic mass of the electron, its angular momentum will be proportional to $ma^2\omega$. Thus, the magnetic moment will be proportional to the angular momentum multiplied by e/m. The quantum theory indicates $h/2\pi$ as the angular momentum, and *assuming the factor of proportionality to be* $\frac{1}{2}$, the magnetic moment is found to be $92 \cdot 7 \times 10^{-22}$ E.M.U.

The magnetic moment of the magneton of Weiss is $18 \cdot 54 \times 10^{-22}$, which is exactly 1/5 of the number given above. It is to be noted that the magneton of Weiss "is not in any way identified with the electron, but is an empirical quantity derived directly from the magnitudes of the susceptibilities of paramagnetic elements and compounds, and for such substances only; it has no meaning for diamagnetic substances." (Parson, p. 76). Thus, the magneton is purely empirical, and not a mechanistic conception. In another place,* however, the author has suggested that it may arise as a difference effect.

Parson has given an estimate of the radius of his magneton, starting from the assumption that the positive sphere of a large atom is nearly proportional to its "magneton number." He concludes that the radius of the magneton is about $1 \cdot 5 \times 10^{-9}$ cm. Assuming further that the velocity at the circumference of the magneton is equal to the velocity of light, the magnetic moment $= \frac{1}{2}eac = 3 \cdot 5 \times 10^{-19}$ E.M.U. This is far larger than the magnetic moment of the Iron atom in the metal at saturation for which the value is 2×10^{-20} only; but on Parson's theory no atom, however many magnetons

* H. S. Allen, "Phil. Mag.," Vol. XXIX., p. 718, 1915.

it contains, can have a moment greater than that of one magneton, and the moment of most atoms will be very much less than this, because the force between the magnetons in an atom will always tend to orient them, so as to make their resultant moment zero.

S. B. McLaren * found that a magneton of any cross-section or aperture has an angular momentum about its axis of $(8\pi^2c)^{-1}N_eN_m$, where c is the velocity of light, N_e is the number of tubes of electric induction terminating on the surface, and N_m is the number of tubes of magnetic induction passing through the aperture. The applications which McLaren proposed to make "to the theory of complete radiation, spectral series, and the asymmetrical emission of electrons in ultra-violet light" were apparently never published.

Conclusion.

If the arguments in favour of a ring electron prove convincing, an important conclusion follows as to the central portion of the atom. The facts of radioactivity indicate that β particles have their origin in the nucleus of the atom. If, then, ring electrons are present near the centre of the atom it will be necessary to revise the prevailing view as to the small size and purely electrostatic field of the nucleus. I have suggested previously that the core of a terrestrial atom is large enough to exert appreciable magnetic forces. This is a natural assumption to make when the core contains both ring electrons and positive units, whatever may prove to be the nature of the latter. In this case it is no longer necessary to postulate the large positive sphere of the Kelvin-Thomson atom, which has been employed in Parson's theory. When the effect of the magnetic core is taken into account, the difficulty found by Parson of reconciling Moseley's atomic numbers with the number of magnetons in the atom disappears.

"To the physicist a theory is a policy rather than a creed." The ring electron not only serves the purpose of explaining known results, but suggests further lines for investigation and experimental research.

In conclusion, I should like to emphasise the fact that when electrostatic forces alone are considered, it is impossible

* "Phil. Mag.," Vol. XXVI., p. 800, 1913; "Nature," Vol. XCII., p. 165, 1913.

to secure stationary electrons such as seem required in order to explain the facts of valency and stereochemistry, and the results of Compton and Rognley.

On the other hand, when magnetic forces alone are considered it is necessary to postulate molecular magnetic fields of extremely large intensity (10^7 gauss, Oxley and Weiss).

These difficulties may be overcome at one and the same time by assuming the action of both electrostatic and magnetic forces in connection with the core of the atom and the electron. We may then introduce stationary electrons exerting both electrostatic and magnetic attractions and repulsions. The energy of the molecular field would be partly electric partly magnetic.

It remains to reduce the theory to a quantitative form by determining the magnetic moment of the ring electron and that of the core of the atom, and then separating the local molecular field due to the magnetic action from that due to electrostatic action.

ABSTRACT.

Dr. H. S. Allen discussed the arguments in favour of an electron in the form of a current circuit capable of producing magnetic effects. Then the electron, in addition to exerting electrostatic forces, behaves like a small magnet. The assumption of the ring electron removes many outstanding difficulties :—

(1) There is no loss of energy by radiation as in the case of a classical electron circulating in an orbit.

(2) Diamagnetic atoms must have a zero resultant magnetic moment. This is difficult to account for with electrons in orbital motion.

(3) The ring electron gives a good explanation of the facts of paramagnetism, including the experimental results of K. T. Compton and Trousdale, and of A. H. Compton and O. Rognley obtained by X-ray analysis.

(4) The asymmetry of certain types of radiation can be accounted for (A. H. Compton).

(5) The effect of the magnetisation of iron upon its absorption coefficient for X-rays observed by Forman is explained.

(6) The small amount of ionisation of gases produced by X-rays may receive an explanation.

(7) Grondahl claims to have found evidence for a magnetic electron in certain thermoelectric effects.

(8) Webster has given a method of deducing Planck's radiation formula by making certain assumptions as to the internal mechanism of Parson's " magneton."

(9) It is suggested that Bohr's theory as to origin of series lines in spectra may be restated so as to apply it to the ring electron. The essential points of the quantum theory and Bohr's equations may be retained, even if his atomic model be rejected.

(10) If radiation is due to pulsations in a ring electron the Zeeman effect may be deduced by reasoning similar to that first employed by Lorentz.

(11) The scattering of streams of electrons from the sun due to electrostatic forces would be to some extent diminished.

(12) Parson has shown that many of the problems of chemical constitution and stereochemistry may be solved by a magneton theory of the structure of the atom. *Stationary* valence electrons are possible.

(13) The forces of cohesion in a solid are similar in nature to chemical forces, both sets of forces having an electromagnetic origin.

The questions of the mass and magnetic moment of such a ring electron were discussed. It was pointed out that the adoption of this hypothesis would lead naturally to the acceptance of an atomic model with a magnetic core as previously suggested by the speaker.

DISCUSSION.

Dr. D. OWEN expressed regret at the unavoidable absence of Prof. Nicholson. His contributions on the subject of atomic structure have already won an important place, and his participation in this discussion had been looked forward to with much interest. Dr. Allen has marshalled a number of lines of argument in favour of the conception of the ring electron. The evidence on the whole scarcely impresses as convincing. Whilst from the point of view of certain phenomena a strong case may be made for its acceptance, it seems equally true that in regard to other phenomena a strong case may be made against.

The hypothesis of the ring electron was proposed by Parson in 1915, in a paper eminent for its lucidity of style, and for the fairness of treatment of prior views of the electron, and it related problems which arise in the investigation of atomic structure. The hypothesis meets with its most striking success on the side of its chemical application. It opens a wider range of interpretation of the chemical bond, explaining, for instance, not only the attraction of different atoms as in a molecule of HCl, but also the attraction of atoms of the same kind, each electrically neutral, as in a molecule of hydrogen. The property of high stability of the group of eight ring electrons arranged in cubic order is decidedly interesting, and appears to meet in a simple and satisfactory way the Law of Octaves apparent in the arrangement of the atoms in order of ascending atomic weight. Another feature is that places are reserved for six elements lying between hydrogen and helium. It is unfortunate that none of these places has yet been filled. The one immediately below helium, termed protofluorine, might well have been expected to reveal itself in virtue of its possession of a unit valency. Parson, however, is only able to remark that its absence must be assigned to causes at present unknown. It would be premature to claim Coronium and Nebulium for any of these places.

Again, in reference to radiation, one of the chief advantages claimed for the ring electron is that it avoids the difficulty arising from the excessive rate of radiation of the atoms of the lightest elements on a particle-electron hypothesis. The ring electron will not radiate at all. But then the atom does radiate! How is the ring electron to permit of this? Not, apparently, without the aid of supplementary hypothesis, tending to rob the hypothesis of its engaging simplicity. The conclusion may perhaps now be accepted as certain that there are circumstances in which the Newtonian mechanics do not hold (*vide* Jeans' Report on

Radiation and the Quantum Theory, published by this Society). This is a general consideration which may have application in solving the difficulties connected with radiation as treated by the ordinary electrodynamic theory.

Judged by its power of predicting new phenomena, the hypothesis of the ring electron is disappointing. Apart from the insertion of new elements of low atomic weight, the fresh phenomena which it points to are only of a secondary order, difficult to place in evidence and of little consequence if verified. In striking contrast is the fertility of Bohr's hypothesis of the atomicity of angular momentum, leading to the prediction of a hitherto unobserved spectral series, a prediction swiftly verified by the observations of Lyman.

Thus, on the whole, the ring electron cannot yet be said to have shown its capacity of rising to the highly responsible duties imposed upon it by the varied atomic phenomena which it is called upon to explain and correlate.

Dr. W. Wilson said that the question of the energy loss by radiation was not really a difficulty in the case of the classical electron. We were obliged to keep to the quantum theory because it was impossible to deduce Planck's law without departing from the old dynamics, or rather, introducing special types of constraint. Now, on the quantum theory, the electron was capable of travelling in a stationary orbit for finite periods without radiating.

We might introduce ring electrons to account for paramagnetism with another group of electrons giving electrical conductivity.

Prof. Nicholson's contention that co-planar rings of electrons were impossible was founded on the old dynamics; but it was unsound to apply the old criteria for stability. On the basis of the quantum theory he thought it might be possible to have co-planar rings.

He did not think it was possible to draw the conclusion that cohesive forces are electromagnetic from Michelson and Morley's experiment.

Prof. Hale (of Mount Wilson Observatory) said he would take this opportunity of thanking the Physical Society of London for the Honorary Fellowship conferred on him some years ago. He would not venture to discuss the constitution of the electron but would refer to some solar pheomena which might be of interest in connection with it.

There were two classes of magnetic phenomena in the sun. In spots there were intense magnetic fields of 5,000 units or so extending over considerable areas, and there was no very clear explanation of the manner in which the necessary separation of charges occurred. If the separation were considerable one would expect some evidence from the Stark effect. A very careful search for this had been made but without result. With iron they had investigated the Stark effect in the laboratory and attempted to compare it with spot phenomena, and found that the electric field must be less than 200 units.

The general magnetic field of the sun is so small that the total displacement of spectrum lines is only about 1/1,000 A.U., *i.e.*, the maximum intensity is about 50 units. Its polarity is the same as that of the earth, and the magnetic poles are 6½ deg. from the axis. The field diminishes rapidly above the surface. It is undetectable at a height of 1,000 to 2,000 miles.

The President reminded the meeting of the observations of Rutherford and his pupils on the scattering of α-particles by matter. These observations led Rutherford to substitute the atom with a positive nucleus for the Kelvin-Thomson atom. As Dr. Allen had not mentioned the subject he would like to ask whether the ring electron would give the scattering found by experiment.

Mr. J. H. Jeans (communicated): Since the appearance of Weber's theories of magnetism (1852), physicists have had every reason to expect

that the phenomena of magnetism would prove to be traceable to charges of electricity describing orbits of at most molecular magnitude. The recent discovery that not only the charges on electrons are "atomic," but that there is some kind of "atomicity" in their very motions leads naturally to the supposition that some kind of atomicity is to be expected in magnetic phenomena. This atomicity has been revealed by the researches of Weiss and Parson.

It is natural to try to construct a model which shall explain the mechanism of this atomicity. The model which most obviously suggests itself is the ring electron. So long as this model fails to fit in with all known physical facts it cannot be regarded as an expression of ultimate truth; it will remain merely a model, making no claim to uniqueness, and its value will be judged by the number and importance of the facts that it accounts for and by the ability it shows to predict undiscovered phenomena, or to suggest new lines of research.

The ring electron does not appear to have been especially fortunate in this latter respect, and its claim to consideration must perhaps rest mainly on arguments such as those marshalled by Dr. Allen. In considering a claim of this kind it must always be taken for granted that the proposed model will explain at least one or two facts perfectly. The model has been devised *ad hoc*. What we have to examine is whether it explains other extraneous facts, and particularly whether it does so without destroying more of the old fabric of science than it builds of new.

It is to be feared that Dr. Allen's claim that the ring electron does not lose energy by radiation will not help it. An electron of the old-fashioned kind did not lose energy by radiation, unless it was accelerated in space, and a ring electron, when accelerated in space will lose precisely the same amount of energy as the old-fashioned electron. The fact that the ring electron, rotating freely and with its centre of gravity unaccelerated, does not lose energy, is not an argument in favour of the ring electron, because any number of electrons can be devised having the same advantage. But, according to the classical dynamics, the kinetic energy of translation of a system of ring electrons would soon be degraded into radiation, and nothing short of the abandonment of the classical system of dynamics will provide a way out of the difficulty.

Dr. Allen's suggestion that Bohr's theory of spectral series might be restated in terms of the ring electron would seem to require that the radius of the electron should replace the radius of the orbit considered by Bohr. The atom with one electron must consist of a ring electron with a nucleus at the centre of the ring, a rearrangement of the atom which appears to do more harm than good. The nucleus is now brought to rest in the atom instead of describing an orbit of extremely small radius. But it is just the description of this very small orbit by the nucleus that results in the variations of the Rydberg constant. These variations enabled Fowler to determine the mass-ratio of electron to hydrogen nucleus to be 1/1836, a determination which must be regarded as purely illusory if the nucleus is to be put at the centre of the electron. Similarly, the recent work of Sommerfeld on the structure of spectral lines seems to admit of no interpretation in terms of the proposed new conception of the atom.

Prof. Webster and Dr. Allen suggest that the ring electron shows special aptitude for explaining Planck's law of radiation. But in this respect the ring electron is like any other system—if it is assumed to obey Planck's dynamical laws, it will give Planck's radiation formula, and if it is assumed to obey the classical laws, it will give the equipartition (Rayleigh-Jeans) formula. Different radiation formulæ in a state of thermodynamical equilibrium are associated with different systems of dynamical laws, not with different systems of mechanical models.

It appears, then, that the ring electron may be welcomed mainly as giving a vivid picture of certain magnetic phenomena. There seems to be no

clearly established case in which it successfully explains any phenomenon outside magnetism, and it is quite out of the question to suppose that the ring electron is going to reconcile the classical dynamics with phenomena which are demonstrably inconsistent with the classical dynamics.

Prof. J. W. NICHOLSON (communicated): Whatever may be thought regarding the existence or non-existence of a ring electron, and whatever our views may be as to the utility of ascribing defined structure to an electron in the present stage of our knowledge, it must be admitted that Dr. Allen, by collecting together these various isolated and sometimes obscure and little known results, which of course in some cases require further verification, has rendered an important service which it has for some time been necessary to undertake. That the same cause, whatever it may be, is the basis of several of them has been rendered fairly clear. Personally I prefer, like Dr. Allen, to keep an open mind, but at least we must take the ring electron seriously on account of the great elegance of the treatment which can be applied to it and of some of its properties. At the same time, I cannot claim to be one of its supporters as a physical entity with the evidence at present available, in spite of the admiration we must all feel for some of the work to which its introduction has led. As regards detailed discussion of Dr. Allen's Paper, I do not feel that I have much to say. He has presented the case very impartially, and indicated where necessary the possible doubt which can be thrown on individual phenomena which have been claimed to exist. He has, in fact, left us very little that can be said on the subject proper, though on other issues which arise in connection with it very much might be said. I must, however, confine myself to one or two points.

One of the most fundamental difficulties of all the more recent views on the atom is that presented by magnetic phenomena. The quantum view is peculiarly at fault here in its present form, and some essential modification must be made. The neglect by all but spectroscopists, of the theory of Ritz on the origin of spectral series—not that I believe in it as it stands—is astonishing. This theory, while on a clear magnetic basis which is quite precise, has predicted spectral series and laws of spectra to an extent which the quantum theory cannot at present approach, and continues to do so. It must be borne in mind steadily by every spectroscopist—and, in fact, is at the basis of every new spectrum discovered as a regular series—while it can, given two series of an element, predict all the others. It can never be superseded as an effective prophet. Bohr's quantum theory is, of course, another prophet which has been effective over a small range. The difficulty is to extend it. Recently it apparently became possible in the hands of Sommerfeld to explain the structures of some of the simpler spectral lines on the hypothesis that the separation in H_β had the same frequency as that in H_α—in other words, that Balmer's series was a Diffuse series. Dr. Merton and myself made an experimental investigation to test this point, published in the "Phil. Trans." It turned out to be untrue. After verifying Michelson and Buisson and Fabry's value for the separation of H_α, we passed on to that of H_β, which was found to be of a different order, and, in fact, of exactly the amount required by a Principal, not a Diffuse series. The present theory of spectral *structure*, as distinct from distribution of lines, is thus very unsatisfactory, and must at least be re-cast in some form not at present evident.

The quantum theory, therefore, is rendered even more tentative, and great caution is necessary in making fresh hypotheses regarding the atom in order to extend its interpretation to other phenomena, though I confess to a vivid interest in such efforts and also to a share in them myself.

Dr. Wilson and others have referred to the vexed question of coplanar rings of electrons, and the point at which ordinary dynamics breaks down in the atom. That a breakdown of the dynamics of matter in bulk does occur is certain, but hardly unexpected when we proceed to the unit of matter. The quantum theory is not, in fact, such a startling revolution as it at first seemed. Those who speculate on the electron, and to a much

less extent the atom, are in the same position as the ancients who speculated on the structure of matter in bulk.

I do not feel justified in going far on this occasion into the coplanar ring question. Its impossibility as an atomic structure is not, in fact, confined to the old dynamics. It permeates most of the obvious extensions of Bohr's new dynamics, from the hydrogen atom to the more complex ones, and is a fundamental difficulty even in the hydrogen molecule. What I have said may, however, be sufficient to show my belief that speculation on such questions as the ring electron is as essential to future development as the raising up of a quantum theory of the atom. We must continue to trace the career of any theory as a prophet until its abandonment becomes imperative, whether we are in any sense a believer in that prophet or not.

Sir Joseph Larmor (communicated): Dr. Allen argues in an interesting way for an amperean molecular permanent current as the ultimate element. In early days that was attractive. In the form of a fluid vortex carrying an electric charge its relations of stability were discussed by Pocklington in the "Philosophical Transactions" about 20 years ago. The objection is that it is not a physical unit. A vortex ring can represent a magnet as regards its field, and the forces between magnets *except as to sign*, but electric attraction is not provided for unless by adding an attracting charge. An electron is a physical conception complete and self-contained. One or more electrons constrained to move round a channel would be like an amperean current. It is not unlikely that constraint of this kind will have to be introduced into molecular models, to give an account of paramagnetism and ferromagnetism—namely, structure in space or atom involving channels more or less definite for the electrons to circulate in.

Dr. Allen, in reply, said that reference had been made to the mass of the positive part of the atom. It was not necessary to postulate a very small nucleus to account for the electromagnetic mass of the whole atom. The core might be complex and contain both electrons and α-particles; or the H atom itself might be built up of positive electrons, and it would be to those that we must attribute the small mass.

As regards scattering, Rutherford's deductions were inconclusive, as he had attended only to electrostatic forces. If magnetic fields were also considered it was possible to account for the scattering phenomena without assuming a very small nucleus. Then there was the question of where the Newtonian dynamics broke down. On Bohr's theory, this must occur somewhere between the electron and the core of the atom. If we adopt the ring electron in conjunction with a magnetic core, we may say that the Newtonian dynamics breaks down within the electron. Personally, he did not accept Parson's theory as it stood; but although the ring electron was essential to Parson's theory, that theory with its distributed positive sphere was not essential to the ring electron.

III. *Relativity and Gravitation.* By WILLIAM WILSON, *Ph.D., D.Sc., University of London, King's College.*

RECEIVED OCTOBER 21, 1918.

THE theory of relativity was greatly simplified by Minkowski,* who perceived that the equations of transformation (the Lorentz transformation) from a system S to another S', moving relatively to S with a uniform velocity, were precisely the extension to a four-dimensional world of a change of axes in three dimensions. The metrical properties of this Minkowskian four-dimensional world are similar to those of three-dimensional Euclidean space. In particular the square of an element of length in it has the form

$$ds^2 = dx^2 + dy^2 + dz^2 + du^2, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where dx, dy, dz, du may be termed the projections of ds on the respective co-ordinate axes.

Recently Einstein,† making use of speculations of Riemann,‡ has given a theory of gravitation the basis of which consists in assuming a more general form for the square of the element of length in the four-dimensional space-time manifold, namely,

$$ds^2 = \sum_s \sum_t g_{st} dx_s dx_t, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$s = 1, 2, 3, 4$$
$$t = 1, 2, 3, 4,$$

where dx_1, dx_2, &c., are now written instead of dx, dy, &c. The 16 coefficients g_{st} are the components of a covariant tensor of the second order, and functions of the x_s. The 16 products $dx_s dx_t$ are the components of a contravariant tensor of the second order. The equations which Einstein deduces for the motion of a particle in a gravitational field are given by Dedekind in a note at the end of one of Riemann's papers.§ What is new in Einstein's work is the application which is made of Riemann's ideas, and also the mode of determining the coefficients g_{st}.

The purpose of the present communication is to show that the equations of motion of a particle in a gravitational field

* "Raum u. Zeit.," published, together with Papers by Lorentz and Einstein, in a volume entitled "Das Relativitätsprinzip," Leipzig, 1913.

† Einstein, "Die Grundlagen d. allgemeinen Relativitätstheorie," "Ann. d. Physik," XLIX., p. 769 (1916).

‡ Riemann, "Ueber die Hypothesen, welche der Geometrie zu Grunde liegen," "Ges. Werke," Leipzig, 1876, p. 254.

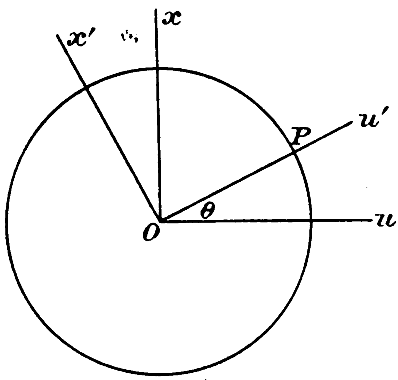
§ Riemann, "Commentatio mathematica," &c., "Ges. Werke," p. 388.

can be put in a Hamiltonian form, that is to say, in the form,

$$\left.\begin{aligned}\frac{dp_s}{d\tau}&=-\frac{\partial H}{\partial x_s},\\ \frac{dx_s}{d\tau}&=\frac{\partial H}{\partial p_s},\end{aligned}\right\} \quad \cdots\cdots\cdots \quad (3)$$

where the p_s are the components of a covariant four-vector, which may be termed the momentum of the particle, and H is a function of the p_s and x_s. The independent variable, τ, is the exact analogue of Minkowski's "Eigenzeit," the meaning of which will appear in what follows.

A very brief account of the older (Minkowskian) form of relativity, more particularly in regard to its bearing on the subject of the present Paper, will be of assistance. Let us suppose there are two systems, S and S', on which are ob-



servers provided with similar physical measuring apparatus, the system S' moving with a uniform velocity, v, relative to S. In order to simplify our statements, let us further suppose that the sets of axes of co-ordinates in S and S' to which the observers refer their measurements are so chosen as to fulfil the following conditions :—

1. They exactly coincide at the time $t=t'=o$, where t and t' represent the time as measured by the observers on S and S' respectively.

2. The directions of the axes are such that the velocity of S' relatively to S is in the (common) x or x' direction.

If, now, we define u by the equation,

$$u^2 = -c^2t^2,$$

where c is a universal constant—the velocity of radiation in empty space, all the postulates of the older form of relativity can, I think, be comprised in the single statement,

$$x^2+y^2+z^2+u^2=x'^2+y'^2+z'^2+u'^2, \quad . \quad . \quad . \quad . \quad (4)$$

where x, y, z, u is a point-instant referred to the system S and x', y', z', u' the same point-instant referred to S'. If, for example, a light pulse is emitted at the time $t=t'=0$ from the common origin of S and S' then we have for any point reached by the disturbance,

$$x^2+y^2+z^2+u^2=0,$$
$$x'^2+y'^2+z'^2+u'^2=0,$$

or,

$$x^2+y^2+z^2=c^2t^2,$$
$$x'^2+y'^2+z'^2=c^2t'^2.$$

The light will spread out in the form of a spherical shell with the same velocity c for both sets of observers; and each set of observers will regard the origin of their set of axes as the centre of the shell.

The simplest and, I think, from the point of view of the physicist, the only satisfactory way of satisfying the postulate (4) consists in putting

$$y=y',$$
$$z=z',$$

and consequently,

$$x^2+u^2=x'^2+u'^2.$$

So that (x, u) are related to (x', u') in the same way as the rectangular co-ordinates of a point, referred to two sets of axes of co-ordinates, in plane geometry.

Consider a point P fixed in S', whose S' co-ordinates are o, y', z', u' (*see* Figure). In the system S its co-ordinates will be $x, y=y', z=z'$ and u. Let θ be the (imaginary) angle between u and u', then clearly

$$\left.\begin{aligned} x' &= x\cos\theta - u\sin\theta, \\ u' &= x\sin\theta + u\cos\theta. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Now, v, the velocity of P relative to the system S, is expressed by

$$v=\frac{x}{t}.$$

Therefore,

$$\frac{v}{ic}=\frac{x}{u},$$

or,

$$\frac{v}{ic}=\tan\theta.$$

It is convenient to use the letter γ to denote $\cos\theta$, so that

$$\gamma=\left(1-\frac{v^2}{c^2}\right)^{-\frac{1}{2}},$$

and

$$\sin\theta=\gamma\frac{v}{ic}.$$

The equations (5) become, on substituting for $\cos\theta$ and $\sin\theta$

$$x'=\gamma(x-vt),$$

$$t'=\gamma\left(t-\frac{vx}{c^2}\right),$$

and if we add

$$y'=y,$$
$$z'=z,$$

we have the equations of the Lorentz transformation. This view of the Lorentz equations as a transformation from one set of axes to another in a 4-dimensional space-time manifold constitutes, I believe, the essential part of Minkowski's contribution to the theory of relativity.

Minkowskian Dynamics of a Particle.

The equations of motion of a particle are

$$\left.\begin{aligned}\mu\frac{d^2x}{d\tau^2}&=f_x,\\ \mu\frac{d^2y}{d\tau^2}&=f_y,\\ \mu\frac{d^2z}{d\tau^2}&=f_z,\\ \mu\frac{d^2u}{d\tau^2}&=f_u,\end{aligned}\right\}\quad\ldots\ldots\quad(6)$$

where μ is an invariant coefficient, which for the sake of simplicity we shall suppose to be constant. It can be identified

with the mass of the particle when at rest or in very slow motion. The quantity, τ, is Minkowski's "Eigenzeit," mentioned above, and is defined by

$$ds^2 = -c^2 d\tau^2.$$

The f_x, f_y, f_z, f_u are the components of a 4-vector—the force 4-vector. We deduce from (6)

$$\frac{\mu}{2}\frac{d}{d\tau}\left(\frac{ds}{d\tau}\right)^2 = f_x\frac{dx}{d\tau}+f_y\frac{dy}{d\tau}+f_z\frac{dz}{d\tau}+f_u\frac{du}{d\tau},$$

and since $\frac{ds}{d\tau}$ is constant we have

$$0 = f_x\frac{dx}{d\tau}+f_y\frac{dy}{d\tau}+f_z\frac{dz}{d\tau}+f_u\frac{du}{d\tau},$$

a result which can be expressed by saying that the force 4-vector is orthogonal to the velocity (or displacement) 4-vector. If we substitute $\mu\frac{d^2u}{d\tau^2}$ for f_u we get

$$-\mu\frac{du}{d\tau}\;\frac{d^2u}{d\tau^2} = f_x\frac{dx}{d\tau}+f_y\frac{dy}{d\tau}+f_z\frac{dz}{d\tau};$$

and remembering that

$$\frac{du}{d\tau} = ic\frac{dt}{d\tau} = ic\gamma,$$

we have

$$\mu c^2\gamma\frac{d\gamma}{d\tau} = f_x\frac{dx}{d\tau}+f_y\frac{dy}{d\tau}+f_z\frac{dz}{d\tau} \quad . \quad . \quad . \quad . \quad (7)$$

It is usual to identify f_x/γ, &c., with the components of the Newtonian force acting on the particle. If we denote them by P_x, P_y, P_z, we have, from (7),

$$d(\mu c^2\gamma) = P_x dx + P_y dy + P_z dz;$$

a result which expresses the energy principle. The energy of the particle is, therefore, if we omit an arbitrary constant,

$$E = \mu c^2\gamma.$$

The product, $\mu\gamma$, is the ordinary or "transverse" mass of the particle. Denoting this by m, we have

$$mc^2 = E,$$

a result first given by Einstein.* The kinetic energy of the particle is

$$\mu c^2(\gamma - 1),$$

* Einstein, "Ann. d. Physik," XVII., p. 891 (1905).

since it must vanish with zero velocity. Since (7) can be put in the form,

$$d\left(\frac{\mu\gamma^2c^2}{2}\right)=f_x dx+f_y dy+f_z dz,$$

the possibility of identifying $\frac{\mu\gamma^2c^2}{2}$ with the energy of the particle suggests itself. This does not seem to have been pointed out before. The kinetic energy would then be

$$\frac{\mu c^2}{2}(\gamma^2-1).$$

Both expressions for the kinetic energy reduce to the ordinary one for small velocities as may be readily verified by substituting $(1-v^2/c^2)^{-\frac{1}{2}}$ for γ. The second expression, it may be noted, can also be written in the form

$$\frac{\mu}{2}\left(\left(\frac{dx}{d\tau}\right)^2+\left(\frac{dy}{d\tau}\right)^2+\left(\frac{dz}{d\tau}\right)^2\right).$$

Motion of a Particle on which no Force is Acting.

This brief outline of the foundation of Minkowski's theory of relativity (as I interpret it) may be concluded by the consideration of a case which, though very simple, is highly important, since it is the exact analogue in the older relativity to the case of the gravitational motion of a particle in the newer or more general form of relativity.

Let p_x, p_y, &c., denote the components of the momentum 4-vector of the particle, so that we have

$$p_x=\mu\frac{dx}{d\tau}, \quad \ldots \ldots \ldots \quad (8)$$

and three similar equations. We may define a function E (not the same as the previous function E) by

$$2E=p_x\frac{dx}{d\tau}+p_y\frac{dy}{d\tau}+p_z\frac{dz}{d\tau}+p_u\frac{du}{d\tau}, \quad \ldots \quad (9)$$

so that
$$E=\frac{\mu}{2}\left(\frac{ds}{d\tau}\right)^2,$$

or
$$E=-\frac{\mu}{2}c^2,$$

and
$$\frac{dE}{d\tau}=0.$$

We may also write

$$E \equiv H = \frac{1}{2\mu}\left[p_x^2 + p_y^2 + p_z^2 + p_u^2\right]. \quad . \quad . \quad . \quad . \quad (10)$$

In the simple case under consideration we have

$$\frac{dp_x}{d\tau} = -\frac{\partial H}{\partial x},$$

$$\frac{dx}{d\tau} = \frac{\partial H}{\partial p_x},$$

together with three similar pairs of equations for the y, z and u axes.

Relativity and Gravitation.

The Minkowskian form of relativity is a special case of the more general relativity introduced by Einstein. In this latter the square of the element of length is expressed by equation (2). Here again we shall define τ by the equation,

$$ds^2 = -c^2 d\tau^2,$$

where c is precisely the same invariant constant as before, though it does not now represent the velocity of light in empty space, except in the special case where

$$g_{st} = 1 \text{ for } s = t,$$

and

$$g_{st} = 0 \text{ for } s \neq t.$$

We shall also define the function E for a particle by

$$E = \frac{\mu}{2}\left(\frac{ds}{d\tau}\right)^2,$$

or

$$E = -\frac{\mu}{2}c^2.$$

The definition of the momentum of the particle (8) must now be replaced by

$$p_s = \sum_t \mu g_{st}\frac{dx_t}{d\tau}, \quad t = 1, 2, 3, 4 \quad . \quad . \quad . \quad . \quad (8')$$

The p_s are the components of a covariant 4-vector. We have therefore

$$\sum_s p_s \frac{dx_s}{d\tau} = \sum_s \sum_t \mu g_{st}\frac{dx_s}{d\tau}\frac{dx_t}{d\tau} \quad \begin{array}{l} s = 1, 2, 3, 4, \\ t = 1, 2, 3, 4, \end{array}$$

and, therefore, by (2)

$$\mu\left(\frac{ds}{d\tau}\right)^2=\sum_s p_s\frac{dx_s}{d\tau},$$

so that

$$2E=\sum_s p_s\frac{dx_s}{d\tau},\quad s=1,2,3,4. \quad . \quad . \quad . \quad (9')$$

If we write out the equations (8′) and solve for $dx_t/d\tau$, we get

$$\frac{dx_t}{d\tau}=\frac{1}{\mu}\sum_s p'_s\frac{G^{st}}{|g|},$$

where $|g|$ is the determinant whose constituents are the g_{st} and the G^{st} are the minors corresponding to the g_{st}. If we write

$$g^{st}=\frac{G^{st}}{|g|},$$

we have

$$\frac{dx_t}{d\tau}=\sum_s\frac{p_s}{\mu}g^{st}.$$

Obviously

$$\sum_s\sum_t g_{st}g^{st}=4,$$

and

$$\sum_s g_{sm}g^{sn}=1 \text{ or } 0,$$

according as m and n are equal or unequal. The coefficients g^{st} are the components of a contravariant tensor of the second order.

If we substitute the expression we have found for $\frac{dx}{d\tau}$ in (9′), we obtain

$$E\equiv H=\sum_\kappa\sum_\lambda\frac{p_\kappa p_\lambda}{2\mu}g^{\kappa\lambda}\begin{array}{l}\kappa=1,2,3,4\\ \lambda=1,2,3,4\end{array}. \quad . \quad . \quad (10')$$

Where the $g^{\kappa\lambda}$ are functions of the x_t. If a few of the terms of H are written out and differentiated partially with respect to p_s we easily find

$$\frac{dx_s}{d\tau}=\frac{\partial H}{\partial p_s},\quad s=1,2,3,4,$$

and since

$$\frac{dE}{d\tau}=0,$$

we have

$$\sum_s\left\{\frac{\partial H}{\partial p_s}\frac{dp_s}{d\tau}+\frac{\partial H}{\partial x_s}\frac{dx_s}{d\tau}\right\}=0\,;$$

whence it follows, on substituting $\frac{dx_s}{d\tau}$ for $\frac{\partial H}{\partial p_s}$ that

$$\frac{dp_s}{d\tau}=-\frac{\partial H}{\partial x_s}, \quad s=1, 2, 3, 4.$$

These are the equations which determine the motion of a particle in a gravitational field. That they are identical with those given by Einstein will now be shown. To begin with, they may be written in the following form :—

$$\sum_t \frac{d}{d\tau}\left(\mu g_{st}\frac{dx_t}{d\tau}\right)+\frac{1}{2\mu}\sum_\kappa\sum_\Lambda p_\kappa p_\Lambda\frac{\partial g^{\kappa\Lambda}}{\partial x_s}=0,$$

therefore $$\sum_t \mu g_{st}\frac{d^2x_t}{d\tau^2}+\mu\sum_t\frac{dg_{st}}{d\tau}\cdot\frac{dx_t}{d\tau}$$

$$+\frac{\mu}{2}\sum_\kappa\sum_\Lambda\sum_m\sum_n\left(g_{\kappa m}\frac{dx_m}{d\tau}\right)\left(g_{\Lambda n}\frac{dx_n}{d\tau}\right)\frac{\partial g^{\kappa\Lambda}}{\partial x_s}=0. \quad . \quad . \quad (11)$$

Since $$\sum_\Lambda g_{\Lambda n}g^{\kappa\Lambda}=\text{constant},$$

therefore $$\sum_\Lambda \frac{\partial g_{\Lambda n}}{\partial x_s}g^{\kappa\Lambda}+g_{\Lambda n}\frac{\partial g^{\kappa\Lambda}}{\partial x_s}=0,$$

and the third term of (11) may be written,

$$-\frac{\mu}{2}\sum_\kappa\sum_\Lambda\sum_m\sum_n g_{\kappa m}g^{\kappa\Lambda}\frac{\partial g_{\Lambda n}}{\partial x_s}\,\frac{dx_m}{d\tau}\,\frac{dx_n}{d\tau},$$

and since $$\sum_\kappa g_{\kappa m}g^{\kappa\Lambda}=1, \quad \Lambda=m,$$
$$=0, \quad \Lambda\neq m,$$

we see that the last term in (11) becomes

$$-\frac{\mu}{2}\sum_m\sum_n\frac{\partial g_{mn}}{\partial x_s}\cdot\frac{dx_m}{d\tau}\,\frac{dx_n}{d\tau}.$$

Equation (11) therefore becomes

$$\sum_t\mu g_{st}\frac{d^2x_t}{d\tau^2}+\mu\sum_t\sum_r\frac{\partial g_{st}}{\partial x_r}\,\frac{dx_r}{d\tau}\,\frac{dx_t}{d\tau}-\frac{\mu}{2}\sum_m\sum_n\frac{\partial g_{mn}}{\partial x_s}\,\frac{dx_m}{d\tau}\,\frac{dx_n}{d\tau}=0 \quad (11')$$

This may be written in the following more symmetrical way :

$$\sum_t\mu g_{st}\frac{d^2x_t}{d\tau^2}+\frac{\mu}{2}\sum_m\sum_n\left\{\frac{\partial g_{sm}}{\partial x_n}+\frac{\partial g_{sn}}{\partial x_m}-\frac{\partial g_{mn}}{\partial x_s}\right\}\frac{dx_m}{d\tau}\,\frac{dx_n}{d\tau}=0. \quad . \quad (12)$$

Apart from immaterial details, this is the form in which the equations are given by Einstein and also by Dedekind in the note mentioned above.

If we write the function E in the form

$$E \equiv L = \frac{\mu}{2} \sum_{\kappa} \sum_{\lambda} g_{\kappa\lambda} \frac{dx_\kappa}{d\tau} \cdot \frac{dx_\lambda}{d\tau},$$

we see that
$$p_s = \frac{\partial L}{\partial \dot{x}_s},$$

where
$$\dot{x}_s = \frac{dx_s}{d\tau},$$

and the equations of motion of the particle may also be written as follows:

$$\frac{d}{d\tau}\left(\frac{\partial L}{\partial \dot{x}_s}\right) - \frac{\partial L}{\partial x_s} = 0, \quad s = 1, 2, 3, 4. \quad . \quad . \quad . \quad (13)$$

since these equations are identical with (11′). This is the Lagrangian form of the equations.

Summary.

The motion of a particle in a gravitational field is dealt with on the basis of the general theory of relativity, and it is shown that the equations of motion can be stated in the Hamiltonian and Lagrangian forms.

ABSTRACT.

The motion of a particle in a gravitational field is treated from the point of view of the general theory of relativity. It is shown that the equations of motion of the particle can be expressed in the following Hamiltonian form:—

$$\frac{dp_s}{d\tau} = -\frac{\partial H}{\partial x_s},$$
$$\frac{dx_s}{d\tau} = \frac{\partial H}{\partial p_s},$$

where p_s is the s component of the covariant 4-vector momentum, x_s the corresponding positional co-ordinate, and τ the Minkowskian "Eigenzeit." A short outline of the Minkowskian Theory of Relativity is included in the Paper.

DISCUSSION.

Dr. H. S. Allen congratulated the author in expressing Einstein's theory in such a comparatively simple way.

Mr. T. Smith asked what was the physical significance of the quantity H appearing in the equations.

The President asked what physical meaning would be attached to the product $dx_s\, dx_t$ when s and t were different.

Dr. Wilson, in reply, said that the quantity H had the dimensions of energy, and occupied a position in the 4-dimensional space-time manifold, similar to that of the Hamiltonian energy function in classical dynamics. He suggested the possibility that the s component of the co-variant 4-vector momentum p_s of an electron may be equal to eA_s, where A_s is the 4-vector potential in the neighbourhood of the electron and e is its charge. He could not, without taking up a great deal of time, explain the meaning of the terms $g_{st}\, dx_s\, dx_t$, $t \neq s$, in the expression for ds^2.

An Exhibition of Some Experiments on Colour Blindness. *By* Mr. C. R. Gibson, *F.R.S.E.*

The apparatus consisted of a lantern to produce a bright beam of white light and a coloured glass which could be slipped in front of it, so as to cut out all the red rays. Various samples of coloured cloths and ribbons arranged in pairs, while quite dissimilar when viewed by the white light, appeared perfect matches with the screen interposed, the conditions then being similar to those in the case of a red blind person. He had found in experimenting with wools by this method that many coloured wools were unsuitable for the purpose on account of fluorescence. Thus, although no red light fell on them from the apparatus, there was plenty of red in the light reflected by the wools. In these cases, in order to see what the colours would appear to the red blind man, it was necessary to have the filter between the wool and the eye, and not simply between the source of light and the wool.

DISCUSSION.

Mr. C. C. Paterson mentioned that in the case of signal lamps it was possible to tell red from green, quite apart from their colour, by looking at them indirectly. A green light got brighter off the line of vision, while a red light got fainter.

Mr. J. Guild asked what screen was employed.

Mr. Gibson said that any screen which cut out the red was suitable. He had tried gelatine dyed with the Sanger-Sheppard minus red. This was good enough for visual work, but not dense enough for the lantern. What he used was practically the ordinary "signal green" glass.

IV. *Note on the Linguistic Nomenclature of Scientific Writers.* *By* ALBERT CAMPBELL, *B.A.*

RECEIVED NOVEMBER 22, 1918.

MOST scientific people are agreed as to the importance of clear and consistent nomenclature, for it is generally recognised that irregularity in nomenclature is a great stumbling block to the learner and even to the expert. In mathematics, particularly for the fundamental processes, regularity is of the first importance; it would be intolerable, for example, if we wrote the squares of a, b, c.. as a^2, b^2, c^2.., but those of x, y, z.. as x_2, y_2, z_2... And yet, when we come to linguistic forms, we find that British scientific writers, instead of helping and hastening the natural tendency of our language to become more regular, are actually the chief offenders in introducing irregularity. Except for the barbarous spelling of its written representation (condemned by all modern philologists), our English tongue is fortunately one of the most regular in grammatical form, and this property gives it world-wide power, for it makes it so easily assimilable by other races. But our British scientific writers, when they introduce new terms from Latin or Greek, leave them as unnaturalized aliens, and persist in retaining their foreign plurals, even in cases where the man in the street has already anglicized them.

They say "media, stigmata, formulæ, lacunæ, genera, radii vectores, foci, in vacuo, quanta," and so on; while in ordinary life we say "mediums, formulas, parabolas, premiums, ultimatums, pendulums, in a vacuum, semicolons, crocuses, geraniums, chrysanthemums, syllabuses, (omnibuses), &c." This medieval practice of our writers is certainly confusing to any student who has not had the advantage of a classical education. It is favoured most of all by persons having a smattering of foreign languages or a tendency to pedantry. Anyone who has learned many foreign languages will naturally feel respect for the regularity of our own. In medical science it is obvious that a jargon is useful for the purpose of necessary camouflage, but physicists at least should show a good example by dismissing from use all such pedantic forms. The French, with their instinctive neatness and logicality, are far before us in this respect; they assimilate consistent nomenclature without the slightest difficulty. We have plenty of philo-

logists who, like botanists, observe all kinds of growths, good, bad or indifferent, but we have no language-gardeners who make it their business to trim the hedges and pull up the weeds; our printers and editors, mostly true watch-dogs of reactionary conventionalism, would turn them out as trespassers.

In this connection I should like to add the following

Note on a Name for $2\pi \times$ Frequency.

In English a name has not yet been found for $2\pi n$, where n represents frequency. Many English writers write $2\pi n$ as p, but ω (already used for it when actual angular velocity is implied) is coming to be its internationally used symbol. French writers call it the "pulsation," while the Germans use the term "Kreisfrequenz." The latter word seems fitting, and is more or less self-explanatory; but the term "pulsation" has already a fairly definite meaning, and a careful terminologist ought to limit the meaning of words rather than extend them. I would suggest that it might be called "*pulsatance.*" The termination "ance" brings it into line with words like inductance and reactance. "Pulsatance" appears a convenient and distinctive term; but, however suitable it may be, it stands a very poor chance of being adopted, when we recognize how much the growth of our language has been arrested. During the last 150 years both French and German have very largely increased their vocabularies, while English (relatively) has stood still, except for the addition of technical terms. This deplorable inertia has greatly impeded the introduction of clear and distinctive scientific nomenclature, and it must be got rid of if our scientific language is to keep pace with our advance in knowledge and precision.

ABSTRACT.

The Note insists on the importance of clear and consistent nomenclature and the avoidance of foreign plural forms, such as *media*, *genera*, *radiivectores*, &c. The term *pulsatance* is suggested as a suitable name for $2\pi \times$ frequency.

DISCUSSION.

Mr. A. P. TROTTER said that he often thought the valuable work done by Prof. S. P. Thompson, Mr. Duddell and Dr. Russell in connection with the nomenclature of electrical engineering might have been taken up in connection with other sciences. The Americans were coining new scientific terms much too fast—in photometry, for instance—and many required careful definition and the restriction of their use to their proper spheres. The work on the Electrical Engineering Nomenclature Committee had been

most interesting, and he thought a small committee of the Physical Society could profitably take the matter up.

Dr. A. RUSSELL congratulated Mr. Campbell on raising an interesting question. Personally he was indebted to Mr. Campbell for many words. As regards classical plurals, it was always rather unpleasant to him to hear words like memorandums, radiuses, &c. Lemma had been mentioned by Mr. Campbell; he always regarded this as being an English word as much as Greek, and thought in most cases it should be left to the literary sense of an author which form of plural to use. As regards ω, this he always looked on as the angular velocity of a rotating vector, and had never found it necessary to use a specific word for it.

Prof. G. W. O. HOWE pointed out that one could not correctly say "an alternating current with an angular velocity ω," since the angular velocity had no reference to the physical alternating current, but only to its graphical representation; one could avoid the difficulty by saying that the current had a frequency $\omega/2\pi$. Some years previously, when he had been on the staff of a firm in Berlin, there was a strong feeling among a number of German engineers against the introduction of non-Germanic technical words. The word Telephon was replaced by Fernsprecher, and objection was taken to such words as booster, the synthetic German compound word being insisted upon. As an indication of the interest taken in such questions, it may be mentioned that, by a mutual agreement among the members of the staff of one department of the works, anyone who at the luncheon table used a word which was not of pure German origin was fined for the benefit of a common fund.

Prof. A. W. BICKERTON said that he had found a serious deficiency in words when working out his new cosmic generalisation, and had found it convenient to coin several new terms. For instance, he had used the word *kinetol* to represent the kinetic energy of unit mass, and *thermatol*, the amount of thermal energy per unit mass, &c.

Mr. C. C. PATERSON said that the termination *-ivity* to indicate a specific quality of a material was one that should be more widely employed. He thought the termination *-ance* should always be carefully limited in its application. It was dangerous to limit the meanings of all words too closely, as there are then no words left for general conceptions. The Americans have already used up nearly all the available terms, and earmarked them for particular meanings. Almost the only one not so treated is *light*.

Prof. LEES said that we appeared to delight in language irregularities. For instance, probably everyone was agreed on the pronunciation of the words *nation*, *national*, but what about the similar pair *ration*, *rational* ?

Mr. T. SMITH (communicated remarks) considered that the note called attention to a matter which had been neglected to a most unfortunate extent. He must plead guilty to having regularly used some of the plurals to which the author took exception, the forms used being, so far as he had noticed, those most frequently employed. A liberal-minded committee charged with the duty of admitting and systematising new scientific words and encouraging their use would be of great assistance at the present time, but there would unfortunately be a considerable risk that a committee appointed for this end would contain a strong conservative element, which would perhaps tend to grow stronger as time went on whose object would be to strangle words struggling for recognition An illustration of the unfortunate tendency in English to combine two words: on one of which a large variety of meanings was thrust, rather than to coin short new words for fundamental ideas, could be drawn from optics. The word *power* was used by itself to denote a definite property of a lens; the same word was also used in *resolving power*, there being no connection between the *powers* in the two cases. Again, *magnifying-power* was used of telescopes and microscopes with different meanings in the two cases. Many other illustrations

could be given, but it was clear that the time had come when an attempt should be made to deal with the matter systematically.

Mr. CAMPBELL, in reply, said he had been much interested in the various comments. He would like to draw Mr. Trotter's attention to the work which a nomenclature committee of this society had already done. Dr. Russell's remarks mainly amounted to saying that having done a thing habitually he did not like to change the habit. But very little practice gets over the repugnance. As regards Mr. Paterson's complaint about over-limitation, there were at present too many loosely used words in scientific terminology. For instance, *pressure* had been stolen from dynamics, and used to mean voltage or potential difference. If chemists were to use the same name for different things in this way there would be frequent cases of poisoning. He agreed with Prof. Howe's objection to calling 2π frequency an angular velocity. He was glad to hear Prof. Bickerton's remarks on the need of new terms in astrophysics

V. *A Note on Low-Frequency Microphone Hummers.* By ALBERT CAMPBELL, *B.A.* (*From the National Physical Laboratory.*)

RECEIVED NOVEMBER 22, 1918.

As many people have tried without success to make microphone hummers that would give low frequencies (of the order of 50 to 100 ∾ per sec.), I should like to state the main conditions which I have found to give successful working at the lower frequencies.

1. The natural frequency of the (loaded) diaphragm of a capsule microphone is high, say 1,000 ∾/sec. For the lower frequencies it is desirable to load it with considerable added mass (*e.g.*, a disk of 200 gm.) in order to bring the natural frequency nearer that which is to be generated.

2. The circuit of the maintaining electromagnet which is connected to the secondary of the transformer should have in series a condenser of sufficient capacitance to bring the effective natural frequency of the circuit somewhat near the frequency of the vibrating bar or fork.

For 50 ∾/sec. a capacitance of 20 to 30 μF may be required; the condenser may, however, be made up of a number of small telephone condensers which are very cheap.

In illustration of the effect of tuning the maintaining circuit I may mention the behaviour of a certain microphone hummer having as vibrator a tuning fork of frequency 100 ∾/sec. When the capacitance is sufficiently large the hummer gives 100 ∾/sec. quite steadily, but if the condenser is cut out entirely, the frequency immediately rises to 666, the first harmonic of the fork, and the fundamental entirely disappears.

3. It should always be kept in mind that the ordinary granule microphones (like the "solid-back" type) usually work best when the plane of the diaphragm is not vertical but tilted at a certain angle (sometimes about 40°) to the vertical. In general it is well to mount the microphone capsule in such a way that the angle of tilt can be varied so as to obtain experimentally the best position.

ABSTRACT.

The note describes the conditions of mechanical loading, capacitance and position which the author has found give successful working at low frequencies.

VI. *A Simple Tuning Fork Generator for Sine-Wave Alternating Current.* *By* ALBERT CAMPBELL, *B.A.* (*From the National Physical Laboratory.*)

RECEIVED NOVEMBER 22, 1918.

AN electrically maintained tuning fork may be used as a source of intermittent current of steady frequency, but the wave form is very far from sinoidal, though no doubt it could be improved by a suitable system of electrical tuning. When only quite a small amount of power is required, the following arrangement forms a convenient source of low frequency current of approximate sine wave form.

To one limb of a fork that can be maintained in the ordinary way there is fastened a small thin coil with its axis at right angles to the direction of motion of the limb. When the fork is vibrating the small coil tends to enter the interpolar space of a permanent magnet fixed to the stand. The voltage induced in the small coil is of approximate sine wave form, and can be varied in amount by altering the position of the magnet or the amplitude of vibration of the fork. The amplitude can be observed by any of the ordinary methods. The total voltage will always be the same when the amplitude and the position of the magnet are fixed. This is convenient for some purposes. Usually the other limb of the fork has to be loaded with a mass similar to that of the small coil. By suitable design of the magnet it is probable that a very pure wave form could be obtained. In the apparatus shown the fork has a frequency of 10 ∾ per sec., and as it has been used in conjunction with vibration galvanometers of this frequency, a few remarks on their behaviour may not be out of place here.

A vibration galvanometer for this low frequency can easily be made to give high sensitivity. For example, with an effective resistance of 1,000 ohms the sensitivity may be 400 mm. at 1 meter distance for 1 microampere. In observing the band of light for a frequency of 10 ∾/sec., the flicker is very evident, and its presence is advantageous, for, when using a null method, the absence of visible flicker is a very sensitive indication of exact balance. The flicker disappears on the scale when the vibration of the spot is reduced to about 0·1 mm. This was observed with moderate illumination and

a scale of distance 2 meters. There is almost the same sensitivity when the light from the mirror is received directly by the eye without any scale at all.

ABSTRACT.

The arrangement consists of an electrically maintained tuning fork, to one prong of which is attached a small thin coil with its axis perpendicular to the direction of motion of the prong. As the fork vibrates the coil oscillates in the field of a fixed horseshoe magnet and an approximately sinusoidal E.M.F. is set up in the coil The frequency with the apparatus shown was 10∿ per second.

VII. *A Method of Comparing Tuning Forks of Low Frequency and of Determining their Damping Decrements.* By Albert Campbell, *B.A.* (*From the National Physical Laboratory.*)

Received November 22, 1918.

For tuning forks of low frequency (below 100 ∼/sec.), the audibility is not good, and comparison by the ordinary method of beats is not easy, particularly as the beats may be very slow and hence the counting inaccurate.

The difficulty may be got over by the following simple arrangement. If the two forks to be compared are provided with electromagnets for maintaining them, the windings of these magnets are put in series and connected to a sensitive vibration galvanometer tuned to a frequency near that of the forks. If the forks are set into vibration the beats are clearly shown by the pulsations of the band of light on the scale, and they can be counted with ease and precision. It is best to get the induced voltages (from the electromagnets) nearly equal, for then at each pulsation the galvanometer deflection comes down practically to zero, which gives a point that can be very accurately observed. It is usually not difficult to attain this condition by slightly stopping down the fork that is too vigorous.

The external circuit should be of such resistance that the galvanometer is considerably damped by it; the deflection should fall promptly to zero when the vibration of the forks is suddenly stopped. The above arrangement assumes that the forks are magnetized, which is usually found to be the case with forks that have been electrically maintained. For forks without maintaining electromagnets, the easiest method is to remove the ear-pieces and diaphragms from two telephone receivers and mount their magnets near one limb of each fork. It is improbable that this will seriously affect the frequencies, but I have not yet made any tests on this point.

The vibration galvanometer method is of course applicable to the comparison of steady electric frequencies from any two sources, such as alternators or audions. To compare the frequency of such a source with that of a tuning fork, it is only necessary to induce a very small voltage from the source into the circuit containing the fork magnet and the galvanometer. Two maintained forks similarly may be compared.

Damping Decrement of Fork.

If the magnet of a single fork is connected to the galvanometer, as the vibration of the fork dies down, the galvanometer deflection falls off similarly. The proportionality is probably very close, but in any particular case might want further investigation.

The sensitivity of the galvanometer can be such that there is a considerable deflection when the amplitude of the fork has fallen to a very small amount.

It might be supposed that the power taken in the galvanometer circuit would appreciably increase the damping of the fork, but the following rough calculation shows how very small the effect would be.

If we assume for the fork an effective mass of 200 gm. vibrating with an amplitude of 2 mm. at $50 \sim$/sec., the maximum velocity will be about 30 cm. per sec., and the energy nearly 10^5 ergs.

If the whole of this energy were uniformly dissipated by the damping in 200 seconds (a near enough supposition to give the order of the effect), the power spent by the fork would be about

$$50 \ \mu W \text{ (microwatts)}.$$

If the galvanometer (not quite in tune) has a sensitivity of 30 mm. at 1 meter for 1 microampere, with an effective resistance of 10 ohms, then for deflection of 60 cm. and 20 ohms in the external circuit the power spent will be about

$$0{\cdot}012 \ \mu W.$$

So the galvanometer circuit takes only about 1/4,000 of the total power lost by damping in the fork; this small effect may in general be neglected.

It can also be shown experimentally that the decrement is practically the same whether the galvanometer circuit is open or closed.

When two forks are compared by the method described above, they form a coupled system linked up by the galvanometer circuit. The preceding calculation shows, however, that the coupling is extremely loose, and it seems probable the mutual influence of one fork on the other is extremely slight. The coupling can be reduced very considerably by working with small deflections, reduced by adding resistance

to the galvanometer circuit. A comparison of the beats with various deflections (for the same amplitude) might indicate the magnitude of the coupling effect if observable.

ABSTRACT.

The method consists in putting the windings of the maintaining magnets in series with each other and with a sensitive vibration galvanometer. The beats are clearly shown by the pulsations of the band of light on the scale.

DISCUSSION ON PREVIOUS THREE NOTES.

Mr. D. J. Blaikley said that in connection with the harmonics or partial tones there was considerable confusion of terminology. Some writers confine the term *harmonics* to those in arithmetical progression with the fundamental. Partial tones not in this series should simply be called partial tones. For instance, in a tuning fork the first partial tone is not harmonic.

Dr. D. Owen thought the vibration galvanometer method for comparing frequency and measuring decrements was much better than any method using a telephone. Why was the method principally applicable to low-frequency forks? With regard to the tuning fork generator, he could not see on what grounds Mr. Campbell states that he gets a sine-wave current.

Mr. I. Williams asked if Mr. Campbell had tried an electromagnet with this apparatus. One could then control the shape of the field, and therefore the wave form of the E.M.F.

Mr. L. Hopwood said he would like to thank Mr. Campbell for the helpful remarks with which the demonstration had been accompanied. If he had had that information early in the war it would have saved him a considerable amount of wasted time.

Prof. Lees said he was interested in the point about the influence of the measuring apparatus on the results. He thought the effect would be very small. We know the amount by which damping modifies frequency and the damping due to the power taken by the galvanometer was very little In connection with the position of microphones he had once been experimenting with a diaphragm. When tilted so that the concavity due to gravity was upwards, it worked regularly. In other positions it was much less regular.

Mr. Campbell said he was interested in Mr. Blaikley's remarks. He was right as to the nomenclature of partial tones and harmonics. Even Lord Rayleigh, he thought, called the tone at 666 the first harmonic of the 100 fork. So did the French writers. In reply to Dr. Owen, he only mentioned low frequencies because the method was the only one suitable for these frequencies. Higher frequencies could easily be done acoustically. With regard to the wave-form obtained with the generator, if one waves a bit of paper rapidly to and fro in front of the galvanometer spot it is clear that the E.M.F. is *nearly* of sine form. This was all he claimed for it at present. With regard to Prof. Lees' observations, he quite realised that the effect of the damping on the frequency of the forks would be very slight, but he was rather thinking of the effect of resonance in pulling the two forks more nearly together. The explanation of the behaviour of the telephone diaphragm mentioned by Prof. Lees might probably throw light on the behaviour of the hummers. He had never known why the tilt was effective.

Prof. Bickerton: Why isn't the horizontal position best, then?

Mr. Campbell: Probably in that case the tighter packing of the granules interferes with the efficient working.

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VIII. *Cohesion* (*Fifth Paper*). *By* HERBERT CHATLEY, *D.Sc.* (*Lond.*).

RECEIVED NOVEMBER 3, 1918.

THE principal purpose of the previous Papers in this series has been to show that cohesion can be approximately represented by the excess of a central force located in the molecules over the kinetic repulsion between them, the said force varying according to a changing inverse power of the distance from centre to centre of the molecule pairs and becoming equal to gravity at the distance of a few molecular diameters.

Dr. Allen in a written communication criticising the fourth Paper expresses the opinion that "It is more than doubtful whether any attempt to found a theory on purely central attractions and repulsions varying as some power of the distance can prove adequate to explain the facts." He also refers favourably to Sutherland's hypothesis, and mentions that Lewis has shown that the Obach Walden relation between molecular pressure and the di-electric constant supports an electromagnetic basis for cohesion. In a previous communication (relating to the author's second Paper) he quotes Weiss as holding an adverse opinion on this magnetic hypothesis.

Jaeger's recent work on surface tension at various temperatures does not wholly confirm the Walden relation.

Sutherland's hypothesis appears (the author unfortunately cannot quote him at first hand) to be wholly electrostatic, and to postulate the attractive forces as due to the non-coincident arrangement of the mutually neutralising charges in a compound molecule, so that the molecule attracts another much in the same way as one of a pair of bar magnets. The writer has made some computations on this basis, and it appears that the force varies inversely as the fifth power of the distance when the molecules are very close, and as the fourth for moderate separation. At large distances the force is zero, and the hypothesis does not therefore explain gravitation. Sutherland has devised the hypothesis that the attraction between unlike charges exceeds the repulsion between like charges in the ratio one plus the reciprocal of 10 to the 43rd power. This means simply that the gravitational effect is superposed and does not appear to afford any explanation whatever of gravitation. The author is under the impression

—possibly erroneous—that cohesive force diminishes with separation more rapidly than is required by Sutherland's hypothesis.

Recent experiments by Svedberg in Sweden and Langmuir and Harkins in America appear to show that electromagnetic forces do play a considerable part in capillarity.

It may even be that the forces are both electrostatic and electromagnetic, the complexity of the vectorisation being explicable in this way.

However this may be, in the absence of a real knowledge of the mechanical structure of a molecule and its constituent atoms approximate formulæ for partially equivalent central forces cannot but be useful, especially if they can be related to the simple electrostatic bonds of nascent atoms on the one hand and the gravitational forces on the other.

For this reason the author adheres for the time being to the form

$$t_2=\frac{Gm^2}{d^{2+n/k}}$$

for the attraction. n being a little more than 4.

For the repulsion he would suggest the following form based on the standard expression for the "virial" or kinetic energy of a molecule and allowing for the actual molecular interstice $(k-1)d_0$,

$$t_1=\frac{RT}{Nd_0}\cdot\frac{1}{3(k-1)},$$

With further reference to the attraction formula, a consideration of all the data afforded by—

(*a*) Strength of materials,
(*b*) Tension of liquid films,
(*c*) Heats of fusion and evaporation,
(*d*) Internal pressure of gases, and
(*e*) Kinetic repulsion of gaseous molecules

points to molecular linkages of the order of 10^{-4} down to 10^{-6} dynes in the solid and liquid states. This then provides the initial values at moderate proximity, and it remains to discuss the rate of decrease with separation.

There is a very distinct relation between the rate of decrease and the ratio of the bond between an isolated molecular pair to the bond exerted by many molecules on one individual. If, for the purpose of hypothesis, we conceive the molecules to be spherical, with densest packing each will be surrounded

by 12 others in mutual contact (the rhombic dodecahedral arrangement) and the attraction exerted by a semi-spherical shell of molecules upon one central one in a direction normal to the diametral plane of the hemisphere will be as follows according to the law of diminution :—

Law of inverse variation.	Ratio of attraction to that of a single pair.
Square ..	Three per shell, integral for indefinite large mass is indefinitely large. This fact decisively controverts the inverse square hypothesis.
Cube....	Three for the first shell, $1\frac{1}{2}$ for the next, one for the third, and so on. Integral not convergent, but only increases slowly.
Fourth..	Three for the first, three-quarters for the second, and so on, as the inverse square of the number. Integral to infinity equals six.
Fifth....	Three for the first, and then as the inverse cube. Integral to infinity equals four.
Sixth ...	Three for the first and then as the inverse fourth. Integral to infinity equals three and four-sevenths.

For the author's formula, with a varying power of the distance, the ratio is practically three—*i.e.*, the innermost shell only is effective.

For cubic packing, the ratios are practically halved (using semi-cubic shells) and for the author's formula only one molecule is effective.

The value of these ratios for real molecules which are certainly not spherical, is somewhat doubtful, but a maximum of three seems clearly indicated. Bragg's results with crystals indicate that the atoms and molecules are immovable in the solid, but a comparison of the diameters deduced from viscosity with those required to fill the volume shows that there is considerable open space. Probably the crystal is a kind of open framework with atoms in effective contact along various lines. The great changes in molecular volume which accompany chemical combination agree with this notion. The high conductivity and small heat capacity at low temperatures as well as the minute expansions from absolute zero to fluidity all seem to show that the atoms are practically in contact at certain points.

IX. *Notes on Lubrication.* *By* S. Skinner, *M.A.*

Received November 18, 1918.

I. Combined Effects of Viscosity and Compressibility in Lubrication.

How two oils of the same viscosity can have different powers of lubrication and why the vegetable and animal oils may under certain circumstances be better lubricants than the mineral oils require explanation. I think the explanation may be in the difference of compressibilities of the various oils. Generally speaking, the vegetable oils are less compressible than the mineral oils.

In my note book I find a short record of some measurements made in November, 1905, on the air pressures in the neighbour-

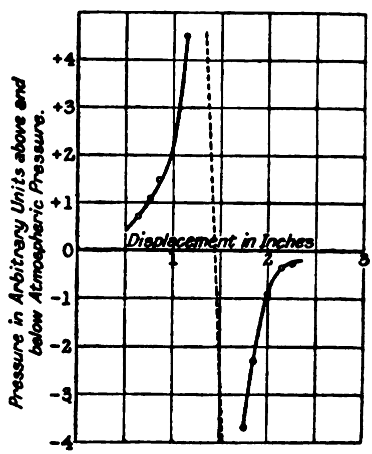
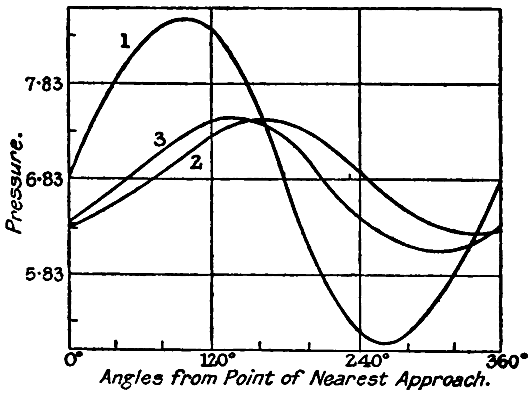


Fig. 1.

hood of a flywheel. A long, flat board had a small brass tube, ending flush with the under surface, passing through it. This was connected to a pressure gauge. The board was placed on the flywheel of a Willans and Robinson engine, which was revolving at 445 revs. per min. The board could be displaced, so that the opening of the brass tube passed over the point of contact of the board and flywheel. Observations were obtained on each side of the point of contact, and are shown in the diagram, Fig. 1. They exhibit the characteristics of lubrication with a compressible fluid.

In Harrison's Paper, Camb. Phil. "Trans.," 1913, an account is given of Kingsbury's experiments with air as a lubricant, and the theory for a compressible fluid is contrasted with that for an incompressible fluid. The following diagram, Fig. 2, is taken from this paper. It represents the pressure of



Speed, 1,730 revolutions per minute; $6{\cdot}83 \times 10^4$ is atmospheric pressure measured in poundals per square foot.

FIG. 2.

air in the space between a complete cylindrical bearing and a journal.

Curve 1 is the theoretical curve for the air, if incompressible. Curve 3 represents Kingsbury's results, and Curve 2 represents Harrison's theory for a compressible fluid. Taking the positive portions of the curves, the areas are :—

Curve 1.	Curve 2.	Curve 3.
1,437	450	450

The bases of the curves :—

Curve 1.	Curve 2.	Curve 3.
56	50	50

∴ Mean pressures indicated by the curves :—

26	9	9

Comparison of the curves shows that for the incompressible fluid the maximum occurs earlier and the minimum earlier, and that the maximum and minimum are about $2\frac{1}{2}$ times as high as the experimental result. The property of compressibility prevents the reaching of high positive or negative pressures, and shifts the position of the maximum and minimum.

Suppose, now, that we have two lubricants of equal viscosity, but one less compressible than the other. The above remarks would lead us to expect that the first would be a superior lubricant to the second.

In the following table are collected the compressibilities of certain lubricants, and their viscosities. The table shows that the vegetable oils have great incompressibility; in fact, they approach water in this respect. I have not been able to find the corresponding data for animal oils, but I think it is likely that they are in the same order as those for vegetable oils.

—	Compressibility.		Viscosity.
Glycerine	$22{\cdot}1\times10^{-6}$	de Metz.	45 at 2·8°C.
Water	$47{\cdot}4\times10^{-6}$	,,	0·018 at 0°C.
Linseed oil	$51{\cdot}8\times10^{-6}$	,,	......
Almond oil	$53{\cdot}5\times10^{-6}$	,,	0·66 at 20°C.
Olive oil	$56{\cdot}3\times10^{-6}$	,,	0·81 at 20°C.
Castor oil	$47{\cdot}2\times10^{-6}$	,,	25·3 at 0°; 5·8 at 6·5°, 1·2 at 27°
Rape oil	$59{\cdot}6\times10^{-6}$	Quincke.	......
Paraffin	$62{\cdot}7\times10^{-6}$	de Metz.	......
C_6H_{14} (hexane)	$159{\cdot}0\times10^{-6}$	Bartoli.	......
$C_{16}H_{34}$	$75{\cdot}0\times10^{-6}$	,,	......
Vaseline oil ...	......	,,	0·91 at 20°

Dissolving a salt in water increases its resistance to compression. Mixing an incompressible with another more compressible fluid probably improves the lubricating power for certain purposes.

In conclusion, we may say that lubricity is a function of viscosity and compressibility. A good lubricant must possess high viscosity and high incompressibility. Is it not probable that the special property of "oiliness" claimed by technical experts in lubrication for certain oils is the physical property of incompressibility?

II. Cavitation in a Stretched Liquid.

In Worthington's Paper, on the mechanical stretching of liquids, "Trans." Roy. Soc., 1892, there is a note on a "Curious Phenomenon of Adhesion between two Solids immersed in a Stretched Liquid." Worthington says: "Desiring to ascertain whether an air free liquid would adhere under tension, as well to a metal as to glass, I enclosed a small piece of folded sheet copper in a glass bulb, which was then filled with boiled out air-free alcohol. Experiments with this showed that there was strong adhesion to the copper as well as to the glass, pro-

vided the vessel was kept still, but any agitation at once caused the stretched liquid to let go its hold at the point of contact of the copper and the glass. Close attention showed that the copper seemed to 'grow to the glass' at the point of contact, when the surrounding liquid was in a state of tension. This led to experiments on bulbs with smaller glass bulbs inside, and in all cases the loose bulb attached itself to the side of the vessel. The equilibrium was, however, very unstable. The release of the liquid took place on the slightest jar, the bubble always appearing at the contact of the solid with the wall, and the loose piece being generally tossed up when the rupture took place. I succeeded best with one small irregular bulb with a projecting stem; this could be gently waved about in the stretched liquid, while the foot of the stem adhered to a point on the side of the containing vessel (showing incidentally that considerable currents may exist in a stretched liquid)."

Worthington's explanation is based on the hydrostatic compression, due to the attraction of the two solids, on the thin layer of liquid between them. I think that the true explanation of the phenomenon may be found in the laws of lubrication. The small glass bulb is completely surrounded by the stretched liquid. If the bulb is against the side of the tube, and the tube is shaken in such a way that the bulb rolls, the pressure in the liquid near the point of nearest approach, and on the side towards which it rolls, will be increased, whilst on the side from which it rolls the pressure will be diminished. The conditions are similar to those of a cylinder rolling on a lubricated surface. The fall of pressure in such a case may be great enough to produce cavitation, and, if not sufficient, the break in the stretched liquid would naturally occur at this point, where the pressure is lower than the mean pressure. Worthington's experiment appears to me to be an evident illustration of the phenomena of lubrication occurring in a stretched liquid.

III. Delayed Boiling and Cavitation.

The phenomenon described in Section II. would naturally lead to consideration of other cases where somewhat similar conditions exist. One of these is to be found in the action of solid spheres in helping to prevent boiling by "bumping." When a liquid is heated in a very clean vessel it may be raised some degrees above the natural boiling point corresponding to the atmospheric pressure at the time. The liquid then

boils with explosive violence. Various methods are known to the chemist for preventing this bumping, which is a source of much annoyance. One is to introduce air by using fragments of porous earthenware, or small lengths of capillary tube closed at one end. Another method is to put into the flask beads which will roll about.

The action of the beads or marbles in rolling on the bottom of the vessel is similar to the rolling of ball bearings in oil-bath lubrication. When the balls roll in the oil they produce cavitation in the oil behind them. It is not necessary to explain the cause of the cavitating action. It is obvious that this is a case of lubrication.

The super-heated liquid is in an abnormally expanded condition like the liquid under tensile stress. To study this I heated some water in a clean beaker until the air was nearly all expelled, and the water reached the stage when large bubbles of steam are formed at a few points only. A thermometer in the water read 102°C., although the barometer was low on the day of the experiment. A slip of glass too long to go to the bottom of the beaker was placed at an angle across the beaker in the water. When this was rubbed with the round end of the thermometer bubbles of steam started in the wake of the point of rubbing contact. This is where cavitation would occur, and into any cavities thus formed the steam would enter, and owing to the high temperature it would be able to dilate the cavity into a large bubble.

Other experiments were made with a glass marble in a flask of alcoholic potash boiling over a water bath. Whenever the flask was tapped so as to move the marble a burst of vapour occurred on the side from which the marble moved. The same was observed with aqueous sodium hydrate solution. In Poynting and Thomson's "Heat" (Griffin & Co., 1904), page 165, it is stated that normal boiling is probably always associated with the presence of bubbles or cavities. An explanation of the action of bubbles of air is given. I think that from what has been said above the effect of cavities produced by cavitation between solids moving relatively to one another in the liquid should now be clear.

IV. Sounds Produced in Cavitating Liquids.

Air bursting into a vacuum produces sound. It might be expected that under suitable conditions sounds could be produced by allowing air to enter suddenly into the vacuum pro-

duced by cavitation. The phenomenon is not of importance, but seems to add interest.

A lens separated by a drop of oil from a flat glass plate is a suitable arrangement. If this is rocked quickly several times in succession, the oil works out to the side until only a narrow wall of oil separates the vacuum produced by cavitation from the air. Then further rocking is accompanied by the breaking of this wall by the rush of air into the vacuum. This is accompanied by a sharp click. It is surprising how loud the sound is for such a small cavity.

South Western Polytechnic Institute, Chelsea.

June, 1918.

ABSTRACT.

Experiments on the pressure of air in the neighbourhood of a flywheel running in contact with a flat tangential board are described to exhibit the properties of a compressible lubricant. A comparison of the compressibilities and viscosities of the vegetable and mineral oils leads to the conclusion that the special property of "oiliness" is the physical property of incompressibility. In Note II. Worthington's experiments on the adhesion of two solids immersed in a stretched liquid are explained as an illustration of the phenomena of lubrication in a stretched liquid. In Note III. the effect of glass beads, &c., in promoting the free boiling of air-free water is explained by the occurrence of cavitation behind the moving beads, &c., the steam entering the cavities thus produced and dilating them into large bubbles.

DISCUSSION.

Mr. T. C. THOMSEN said that Principal Skinner's experiments were of great interest. It was a pity he had not been able to show his experiments with a glass bearing, in which the formation of cavity at the off side of the axle is clearly shown. The compressibility of fluids had been employed by Ferranti to convert air into a lubricant for very high speed spindles rotating at about 30 or 40 thousand per minute. At these high speeds the air is compressed, and a film is maintained between axle and bearing. He could not agree with the author's view that viscosity and compressibility were sufficient explanation of oiliness. Actual bearings are not perfect, and if the lubricant fails it will do so at certain isolated high spots. These are regions of extreme pressure, and he thought the increase of viscosity which was known to take place at high pressures was important. A research is at present in progress on viscosities under high pressures, and if it is found that animal and vegetable oils have greater increases with pressure than mineral oils it will go a long way to explain the effects. There was another point about oiliness. It had been found that among mineral oils the best lubricants were those with a large proportion of unsaturated hydrocarbons. It is thought that the more of these that are present the more intimately the oil will adhere to a metallic surface. Now some of the animal and vegetable oils are very largely composed of unsaturated constituents, so that this property of adherence to metallic surfaces may readily be greater in these cases. He thought compressibility was of negligible importance. It should be borne in mind that the theoretical work which the author had quoted, and to which the diagram referred, only applied to perfect lubrication, with

nothing but fluid friction coming in. In practice, for this case, lubrication depended only on viscosity. It was when lubrication was only partial, with regions of metallic contact, that the property of oiliness came in. The question of cavitation could only come in at low and moderate pressures.

Capt. HYDE said he was interested in the question from the point of view of aircraft engines. Personally he did not think oiliness was a distinct physical property, but that it depended on the action of the oil on metallic surfaces. As Mr. Thomsen had pointed out, the theories of lubrication mentioned by the author referred to pure fluid friction, and there were no experiments to show that in these circumstances there was any difference between mineral and non-mineral oils of given viscosity. In 99 per cent. of cases there was metallic contact, and that was when oiliness came in. Experiments by Mr. Deely seemed to indicate that the differences lay in the action on the metallic surface. It did not seem to him that the difference between the compressibilities of, say, paraffin oil and rape oil would account for the difference in oiliness. Some experiments conducted at the National Physical Laboratory had revealed the curious fact that when working under severe conditions with imperfect lubrication there was, for the mineral oils a critical temperature of about 50°C. or 60°C., at which the lubrication broke down. No such temperature had yet been found with non-mineral oils.

Mr. C. R. DARLING did not think the experiments described by Principal Skinner on delayed boiling disproved the old explanation of the phenomena. There was always a film of air on glass which was very difficult to remove, and which was not removed by boiling. When actual rubbing takes place, as in the experiment mentioned, some of this may easily be liberated. He suggested trying the experiment with surfaces of quenched quartz.

Dr. BORNS, referring to the experiment in cavitation, asked whether Mr. Skinner had noticed a Paper by Tœpler, on the rupture of liquids between a rolling sphere and a plane plate, published in the "Annalen der Physik" last summer. Tœpler put a drop of liquid on a plain glass plate and placed a lens on the liquid. When the lens was rocked about, the liquid gave way, a series of crescents being formed. Tœpler examined these, and determined the critical velocity and stresses. A platinum wire would prevent delayed boiling. In this case the action must be rather on the lines suggested by Mr. Darling.

Mr. T. SMITH said that the difference in oiliness had been said to come in when the film had broken down. What happened then? Did the liquid reunite?

Capt. HYDE said he did not think it was known yet whether the better lubricant joined up quicker or not. In the case of pistons, he thought that even where you had "lubricated surfaces" there was never an actual oil film present. The lubrication was simply a condition of the surface.

Principal SKINNER said it was not clear how Capt. Hyde could be sure of his temperatures in the critical temperature experiments. In reply to Dr. Borns, he had seen Tœpler's Paper, but was reserving reference to it until a subsequent Paper, which he hoped to present to the Society.

X. *On Sir Thomas Wrightson's Theory of Hearing.* By

W. B. Morton, *M.A.*, *Queen's University, Belfast.*

Received November 22, 1918.

In his recently published book entitled "The analytical Mechanism of the Internal Ear," Sir Thos. Wrightson has put forward a new theory regarding the much debated question of the means by which the ear is enabled to separate out the constituent pitches in a compound aerial vibration. His theory has something in common with that which was proposed by Voigt with regard to the origin of combination tones,* inasmuch as in both theories attention is directed to the geometrical features of the curve which represents the motion or pressure of the air. In Voigt's theory the points of maximum and minimum displacement were shown to lie on certain sine curves having the frequencies of the combination tones, and this geometrical fact was supposed to give rise to sensations of the tones in question. Wrightson takes into consideration these stationary points along with the points where the curve crosses the axis. He groups all these two kinds of points together and calls them "Impulse Points," on the ground that there is "an impulse in the stapes and the liquid moved by it, not only where pressure and velocity become nil, which is synchronous with the crossing points of the pressure air-wave, but also at the crest of the wave where the acceleration of velocity and increase of pressure cease" (*loc. cit.*, p. 93). Having drawn a large number of curves got by compounding simple harmonic vibrations of different frequencies and having examined the spacing of their impulse-points, the author finds that there are in each complete cycle some points whose distance corresponds approximately to the frequencies of the component vibrations, others to the octaves below these pitches, others again to the summation and difference tones. These are all called "effective" points, and are held sufficient to account for the sensations of the corresponding tones. In all the special cases enumerated nearly all the impulse-points are stated to be "effective" in one way or another. The degree of approximation of the spacings to the correct interval is not stated.

* Cf., W. B. Morton and Mary Darragh, "On the Theories of Voigt and Everett Regarding the Origin of Combination Tones," in these "Proceedings," Vol. XXVII., p. 339, 1915.

I have not been able to follow clearly the argument by which the author seeks to establish the effectiveness of the impulse-points in relation to the mechanism of the ear. The purpose of the present note is, first, to indicate a method by which the distribution of the "impulse-points" can be examined without going through the labour of drawing each separate case, and second, to point out some difficulties in the way of accepting as valid the fundamental assumptions of the theory.

Let the compound vibration be represented by

$$y=a \sin mx+b \sin n(x+\delta).$$

We shall suppose $m<n$, and shall call m and n the frequencies of the component vibrations. The points where the curve crosses the axis are given by

$$\sin mx/\sin n(x+\delta)=-b/a.$$

Accordingly if we plot the curve $y=\sin mx/\sin n(x+\delta)$ its intersections with the line $y=-b/a$ will give the positions of the points in question. We can thus, by altering the level of a horizontal line crossing the curve, follow the displacements of the zeroes of our compound vibration-curve, as the relative amplitudes of the components vary while a definite phase-relation, determined by δ, is maintained. It is easy to verify that, if the horizontal line is taken at levels both above and below the axis, we can exhaust all possible cases by letting δ run from o to $\frac{\pi}{2mn}$, in separate graphs.

The curve $y=\sin mx/\sin n(x+\delta)$ goes to infinity at the zeroes of the denominator. We can get a more compact graph by working only with amplitude-ratios lying between 0 and 1, so for cases in which the amplitude of the higher note n is the greater we plot the reciprocal $y=\sin n(x+\delta)/\sin mx$. The two curves can be combined in one diagram on which $y=$whichever of the two expressions is not greater than unity, the two parts of the graph being drawn with a full and a broken line respectively.

The stationary points on the vibration-curve are given by

$$ma \cos mx+nb \cos n(x+\delta)=0, \text{ or}$$

$$m \cos mx/n \cos n(x+\delta)=-\frac{b}{a},$$

and so can be discussed in a similar manner by plotting the graph $y=m \cos mn/n \cos n(x+\delta)$ or its reciprocal.

The accompanying figures show these graphs for the interval of a major sixth $m=3$, $n=5$, with $\delta=0$, 3° and 6°, the curves for the two classes of impulse-points being combined on one diagram. The positions of the zeroes are given by the curves marked with a nought at the top and bottom, the curves giving the stationary points are marked with a cross.

Using these graphs we can trace as follows the displacements of the impulse-points as we pass from one note alone to the other alone, through all mixtures of the two, while the phase-relation remains constant.

The middle line of the diagram is crossed by the full-line curves at equi-distant points corresponding to the zeroes and crests of the lower note existing alone. If a horizontal line, starting from the central position is pushed upwards its points of intersection with the curves shift, showing the effect on the positions of the impulse-points of the addition of more and more of the higher note, the distance of the horizontal line from the axis being the ratio of the amplitude of the higher note to that of the lower. Fresh intersections appear when the moving line meets the loops of the graph and before the top of the diagram is reached (equal amplitudes) the number of points corresponds to the higher frequency. If now the line is brought down again its intersections with the *dotted* lines give the impulse-points when the higher note has the greater amplitude, the height above the axis now being the ratio of amplitude of lower to that of higher. Finally on reaching the axis again we have the spacing corresponding to the higher note alone. By carrying out the same movement on the lower half of the diagram we get the case in which one of the two component vibrations is reversed in phase.

It is possible to make certain general statements regarding the number of points of each class. The fresh points appear when the line is at a stationary value of y.

Considering first the zeroes, $y=\sin mx/\sin n(x+\delta)$ is stationary when $\tan mx/\tan n(x+\delta)=m/n$. It it easy to show that the corresponding value of y lies between m/n and unity, $(m<n)$. For

$$\frac{\sin mn}{\sin nx}=\sqrt{\frac{1+\cot^2 n(x+\delta)}{1+\cot^2 mx}}<1 \text{ since } \cot mx > \cot n(x+\delta)$$

and

$$\frac{\sin mx}{\sin nx}=\frac{\tan mx}{\tan n(x+\delta)}\sqrt{\frac{1+\tan^2 n(x+\delta)}{1+\tan^2 mx}}>\frac{\tan mx}{\tan n(x+\delta)}=\frac{m}{n}.$$

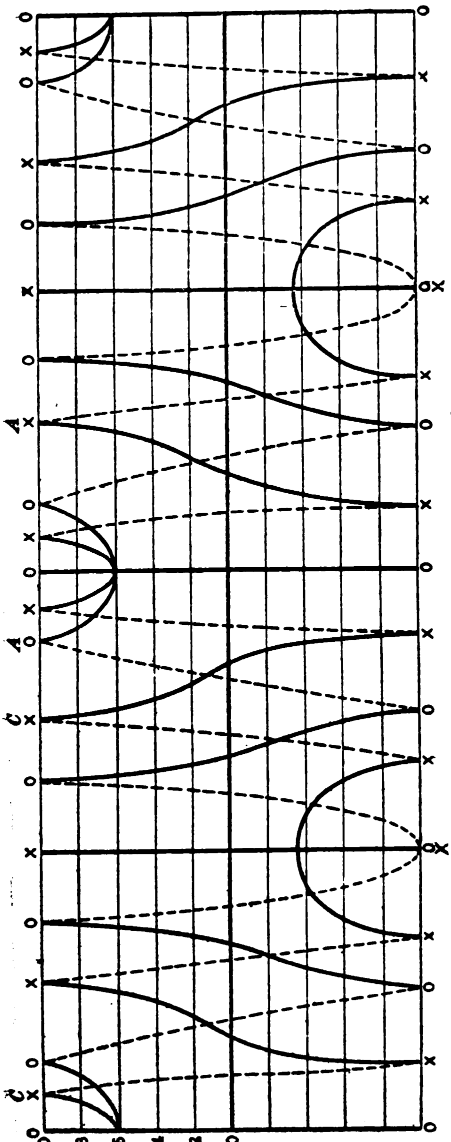


FIG. 1.—IMPULSE-POINTS FOR $y=a \sin 3x+b \sin 5x$.

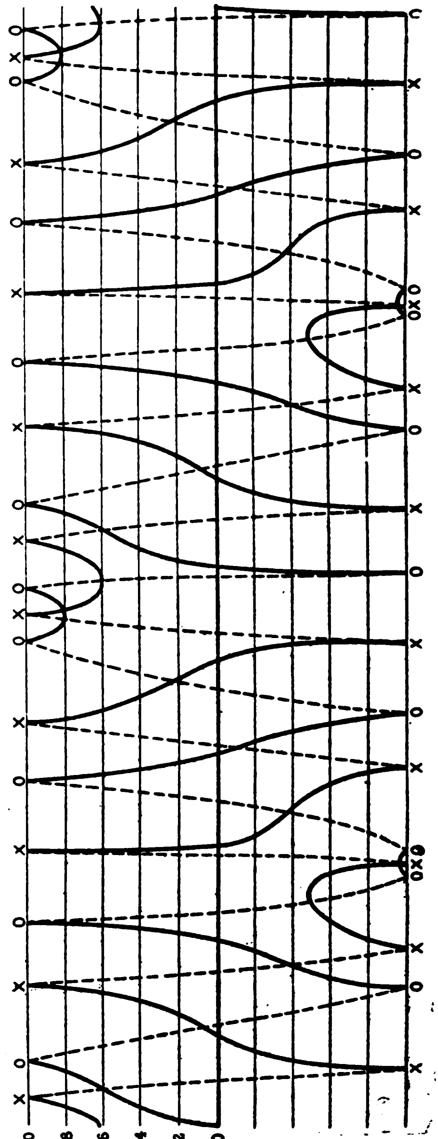


FIG. 2.—FOR $y=a \sin 3x+b \sin 5(x+3°)$.

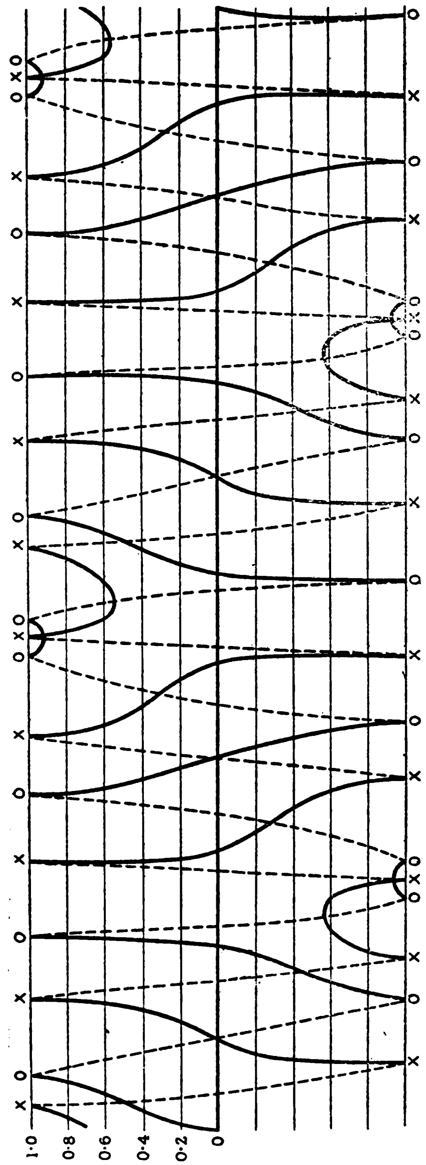


Fig. 3.—For $y = a \sin 3x + b \sin 5(x + 6°)$.

It follows that when b/a, *i.e.*, amplitude of the higher note divided by that of the lower, is less than m/n, the number of zeroes is dictated by the lower note; if this ratio is greater than unity, by the upper note. For values of b/a lying between m/n and unity the number of zeroes on the compound vibration-curve depends on the phase relation-between the component vibrations.

The energies of the two components are as m^2a^2 to n^2b^2, and this may be taken as the ratio of the *physical* loudnesses of the notes. The rule just stated concerning the number of zeroes may then be stated as follows. The lower note dominates when it is louder than the upper, but the upper note does not certainly dominate unless its loudness exceeds that of the lower in a greater ratio than n^2 to m^2.

When the same method is applied to the maxima and minima of the curve, the critical values of b/a are found to be m^2/n^2 and m/n instead of m/n and unity. In terms of loudness this leads to a statement just the reverse of the former one; it is now the upper note which settles the number of points when it is the louder. The lower note does so, for all phase-relations, only when its loudness exceeds n^2/m^2 times that of the higher note.

Sir Thomas Wrightson's theory rests equally on two foundations, viz.: (1) That an impulse is communicated to the ear alike at the maxima and minima of the vibration-curve and at the points where the curve crosses the axis. (2) That the distribution of these impulse-points on the curve is such as to convey the sensations of the pitches which are heard. These two matters can be discussed independently of each other. I confine myself in this note to the second point, assuming for the moment that the supposed impulses actually occur. The following difficulties present themselves.

1. A point is classed as "effective" if it is preceded or followed (but not immediately) by another point at an interval corresponding to the frequency either of one of the two component pitches, or of the octave below either or of a combination-tone. The spacings of the combination-tones may be supposed to give rise to the sensations of those tones, but cannot help the ear to discern the two primaries. Further, there seems no sufficient reason for calling in the assistance of the lower octaves of the primaries. It is stated that these "assist the harmony" (p. 27), but no explanation is offered of the fact that they are not heard as pitches equally with the

primaries. If points reckoned effective by virtue of spacings of these two categories be ruled out, it makes a substantial reduction in the proportion of effective points.

But, independently of this, the fundamental difficulty is to see why the ear should pick out these particular spacings and ignore the others. With so many distances to choose from it is not surprising that a number can be found to approximate to any desired spacing. In order to make out a case for the special effectiveness of a spacing in impressing itself on the ear, it seems necessary to show that it occurs sufficiently often to raise it out of the mass.

I have gone into detail in one special case, the first one used by the author in explaining his theory (p. 26). This corresponds to the top line on my diagram (1), the interval of a major sixth when the notes have equal amplitudes and the two vibrations pass through zero at the same instant in opposite directions, *i.e.*, the case $y=a(\sin 3x-\sin 5x)$. We may call the notes C with period 10, and A with period 6. The complete cycle of the compound curve has period 30.

In taking a census of the spacings presented by this case I confine myself to distances under the half-cycle 15. Taking the 10 points marked with noughts and crosses (zeroes and stationary points respectively) on the left-hand half of the diagram, each of these is the left-hand limit of nine intervals, measured forward, which lie under the prescribed distance, giving $10\times 9=90$ spacings in all. These are made up of 39 different lengths, which are repeated, through the symmetry of the diagram; one occurs six times, six occur four times, 28 twice, and four once. Among those which occur twice are lengths 9·97 and 5·89. The former is a close approximation to C; the latter a worse approximation to A. These spacings are indicated by the letters above the diagram. There are two other lengths, each occurring twice, which lie at about two-thirds of a semitone on opposite sides of C, viz., 9·64 and 10·39. There is no other distance lying within a semitone of A. Thus, among the 90 intervals there are only six which could by any possibility suggest the pitch C and only two to suggest A. On the other hand, there are six exact intervals of 7·5, corresponding to the note F between C and A, which is not heard at all. There are 10 spacings lying within a semitone of the period 11.

It does not help much to bring in the lower octaves. There are two lengths of 12·01 and two of 12·10. To get approxima-

tions to 20 we should have to expend the range of spacings considered, and this would bring in a largely increased number of useless intervals.

2. Another consideration is the alteration brought about in the spacings of the impulse-points when the relative amplitudes are changed. Suppose, for example, that the ear has seized on the spacing marked *CC* in the case just considered, of equal amplitudes. The physical loudnesses of *C* and *A* are then as 9 to 25. Now suppose the loudness of *C* to be gradually increased to equality with *A*. The displacements of the impulse-points can be followed by bringing the horizontal line down to the level marked 0·6 and watching its intersections with the full curves. It will be seen that the two points in question separate to a distance 11·3, corresponding to a pitch below B flat, so this particular spacing would cease to be available long before equality of loudness is reached. The change is still more marked in the case of the points marked *AA*, where there is an approach equivalent to a rise of a fifth.

It may, of course, be urged that as certain pairs of points become gradually unavailable other pairs will become available. But it is hard to reconcile such an inexact and capricious means of determination with the certainty and constancy of the pitch-sensations to be explained.

A similar objection might be based on the alterations in the spacings which are brought about by changing the phase-relation with constant loudness-ratio. This indeed would be a more cogent argument because such progressive changes in phase-relation must always occur except when the tuning is perfect. In any case the actual relation between the phases is a matter of chance and has no influence on the discrimination of the pitches by the ear.

3. The simple harmonic air-motion which gives the sensation of a single pure tone has four impulse-points in each period, viz., two zeroes and two stationary points. In other words if the frequency of the note is n the impulses, according to the theory, fall on the ear with frequency $4n$. One would expect then that also in the case of a compound disturbance the sensation of pitch n would depend on the recurrence of impulses with frequency $4n$, whereas it is sought to be explained by spacings of frequency n.

4. The author adduces as an analogy the case of Seebeck's siren, in which a current of air is directed against a row of

holes spaced round the circumference of a rotating plate. I would suggest that a rough test of his theory might be made if the holes were given the relative positions of the consecutive impulse-points on a compound vibration-curve. This would be analogous to the plan adopted by Koenig when he sought to establish, by his "wave-siren," his views as to the effect of phase-relation upon quality of tone. It would be open to criticisms similar to those urged by Helmholtz against Koenig, but if the arrangement succeeded in giving clearly the two primary pitches this would be recognised as giving support to Sir Thomas Wrightson's views.

The theory has already attracted a considerable amount of attention, specially from those who are dissatisfied with the Helmholtz theory of the mechanism of hearing. Every attempt to discover a fresh point of view in so difficult a matter is to be welcomed.

The book contains a valuable and clear account of the internal anatomy of the ear, in which Prof. Keith has collaborated. Such discussions as I have seen have been directed to the internal processes and have not touched on those preliminary assumptions to which the criticism of the present note is directed.

ABSTRACT.

The theory seeks to explain the power possessed by the ear of analysing into its component tones a compound aerial disturbance. It assumes that (1) impulses act on the mechanism of the ear corresponding to the maxima and minima of the compound vibration-curve, and also to the points where the curve crosses the axis; (2) that among the spacings of these impulse-points there is a preponderance of intervals which approximate to the periods of the component tones, their lower octaves and their combination tones, and that these spacings determine the sensations of the component tones. The present note is concerned with the second of these assumptions. Graphs are drawn which exhibit the way in which the distribution of impulse-points varies when relative intensities and phase-relation of the component notes are changed. Difficulties are found in (1) the large number of other spacings presented to the ear, (2) the variations of the spacings with loudness-ratio and phase relation, (3) the fact that in a single pure tone the spacing is a quarter of the period of the vibration.

DISCUSSION.

Prof. BAYLISS said he would like to make a mild protest against Prof. Morton's statement that physiologists welcomed Wrightson's Theory. It is really the anatomist who says that there is no structure in the ear capable of acting as a resonator, ignoring Helmholtz's demonstration that a membrane stretched unequally in different directions will resonate.

Personally he saw no objection to the resonance theory. On Sir Thos. Wrightson's theory there were four impulse points per vibration, If you reduce the frequency of vibration far enough you cease to hear a sound and hear the separate vibrations. Why do you not notice four impulses per vibration in this case. Another important point is one that Lord Rayleigh raised. We can hear sounds up to 30,000 per second. Now from what we know of nerve fibres, they are completely inactive for at least a thousandth of a second after receiving an impulse so they cannot possibly record more than a thousand separate impulses per sceond. Thus, on Wrightson's theory, all sounds requiring more than 1,000 impulse points per second should sound alike.

Principal SKINNER was interested to hear Mr. Bayliss's opinion that the material of which the body is made is not acoustically the best. He had always had a difficulty in understanding how the cavity of the mouth could act as a resonator at all; the vibrations should be damped down so quickly.

Prof. BRAGG asked if it was not the case that a sound was recognisable if two or three consecutive vibrations were present. This would explain Mr. Skinner's difficulty, for even if the vibrations of speech were very rapidy damped out, there might still be sufficient of them to give the note its characteristics.

XI. *Electrical Theorems in Connection with Parallel Cylindrical Conductors.* By ALEXANDER RUSSELL, *M.A., D.Sc.*

RECEIVED NOVEMBER 26, 1918.

THE electrostatic problem of two conducting spheres having given electric charges and surrounded by a uniform dielectric has been completely solved. The capacity and potential coefficients of the spheres, the density of the surface charges, the potential at any point of the field and the mutual force between them can be computed in all cases without difficulty, to any required degree of accuracy.*

Unfortunately the apparently much simpler problem of two parallel cylindrical conductors has not been completely solved. It will be helpful, therefore, to give simplified proofs of the solutions already obtained and show how they can be extended. It is customary to make the assumptions that the cylinders are infinitely long, and that the other conductors of the system are at a great distance away from them. In this case it is shown that the three capacity coefficients are connected by two simple relations which determine the limits between which they must lie. In certain cases also their approximate values can be found. It is shown how the solution of the electrostatic problem enables us to solve the analogous problem of two parallel cylindrical conductors carrying high-frequency currents. In certain cases the exact values can be obtained of the current density on the surface, the inductance coefficients and the force between the conductors. In other cases useful approximations are given.

The Electric Force at any Point due to Two Parallel Cylindrical Conductors having Equal and Opposite Charges of Electricity.

Let us first consider the case of two thin parallel wires cutting the plane of the paper perpendicularly at A and B (Fig. 1). We shall suppose that they are infinitely long, that they have charges $+q$ and $-q$ per unit length respectively, and that the dielectric is air. To find the force at any point P in the plane of the paper join AP and BP and make the angle BPC equal to the angle BAP. Then PC will be the direction of the resultant force and its magnitude will equal $4qr/(r_1r_2)$ where $AB=2r$, $AP=r_1$, and $BP=r_2$. To prove

* Russell's "Alternating Currents," Vol. I., Ch. VIII., 2nd Ed.

this, we notice that the component force PQ due to the charge on the A wire$=2q/r_1$, the component force PS due to the charge on the B wire $=2q/r_2$, and the angle $PSR=$the angle APB. Hence the triangles APB and PSR are similar, and so the angle BPC equals the angle PAB. Also since $PR/PS=AB/r_1$, PR which is the resultant force, F at P, is given by

$$F=\frac{4qr}{r_1 r_2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The potential v at the point P is the sum of the potentials due to the charges on the two wires, and hence

$$v=2q\log r_2-2q\log r_1=2q\log(r_2/r_1). \quad . \quad . \quad (2)$$

Since the angle BPC equals the angle BAP, the tangent PC to this circle at P gives the direction of the resultant force. Hence the tangent at every point of this circle and

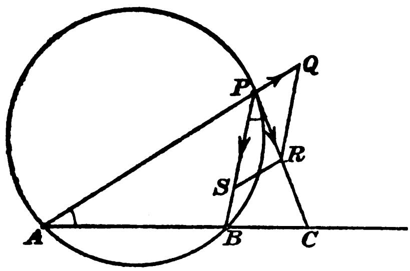


FIG. 1.

PR is the resultant force at any point *P*. The angle *BPC* equals the angle *BAP*. Hence every circle through *A* and *B* is a line of force.

therefore also at every point of any circle through A and B will give the direction of the resultant force. It follows that every circle which passes through A and B is a line of force. Again, if with centre at a point C on AB produced and with radius equal to $(CA \cdot CB)^{\frac{1}{2}}$ we describe a circle, any radius CP of this circle will be tangential to the circular line of force through ABP, and hence this circle will cut all the lines of force at right angles. It is, therefore, the cross-section of an equipotential surface. We see that all the equipotential surfaces round A and B are cylindrical in shape, that their axes are parallel and that the centres of their cross-sections lie on AB or BA produced. The equipotential surfaces surrounding A, we shall call the A cylinders, and those surrounding B the B cylinders. It is to be noticed that if we

take any A cylinder and any B cylinder, A and B are the inverse points of their circular cross-sections.

By Green's theorem we can suppose that any A cylinder and any B cylinder become conductors without affecting the distribution of the flux external to them. Similarly, if two of the A cylindrical surfaces become conducting the distribution of the flux between them will not be affected. The surface density σ also at any point on the surface of these conducting cylinders is given by $F/(4\pi)$, where F is the electric force at the point. Hence the surface density at the point P of the equipotential surface is given by

$$\sigma=\frac{qr}{\pi r_1 r_2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Let the circles in Fig. 2 represent the sections of an A and a B cylinder respectively, and let their radii be a and b. Then, since A and B are the inverse points of the two circles,

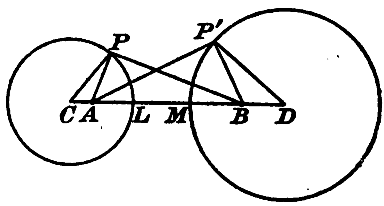


FIG. 2.
A and B are the inverse points of the two circles.

$CA \,.\, CB=a^2$ and $DB \,.\, DA=b^2$, where C and D are the centres of the two circles. The circle described on AB as diameter is the section of the smallest equipotential surface. It cuts the cylinders at right angles. If $2r$ be the diameter of this circle and if we denote the distance CD between the axes of the cylinders by c, we have

$$c^2r^2=4s(s-a)(s-b)(c-s) \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where $\qquad 2s=a+b+c,$ (l.c., *ante* p. 164).

Since $r_2/r_1=BL/AL$, it is easy to show that

$$r_2/r_1=BC/a=a/CA.$$

Hence, by (2), $\quad v_1=2q\log(BC/a)=2q\alpha, \quad . \quad . \quad . \quad . \quad . \quad (5)$

where v_1 is the potential of the A cylinder and $\alpha=\log(BC/a)$.

Hence, $\varepsilon^{a}=BC/a$ and $\varepsilon^{-a}=CA/a$,

and thus $2r=CB-CA=a(\varepsilon^{a}-\varepsilon^{-a})$,

and so $r=a \sinh a$.

Hence,

$$a=\sinh^{-1}(r/a)=\log_{\epsilon}\{r/a+(1+r^2/a^2)^{\frac{1}{2}}\} \quad . \quad . \quad . \quad (6)$$

It will be seen that a can be readily computed by (4) and (6).

We see from (5) that whatever the radius of the B cylinder may be, we have

$$q/v_1=1/(2a) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Similarly, if v_2 be the potential of the B cylinder whose radius is b, we have

$$q/v_2=-1/(2\beta) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

since all the B cylinders have a charge of $-q$ per unit length. The value of β is given by

$$\beta=\sinh^{-1}(r/b)=\log_{\epsilon}\{r/b+(1+r^2/b^2)^{\frac{1}{2}}\} \quad . \quad . \quad . \quad (9)$$

We deduce from (7) and (8) that

$$\frac{q}{v_1-v_2}=\frac{1}{2(a+\beta)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

This is the formula for the capacity between the two cylinders—the capacity usually wanted in practice. Formulæ (7) and (8), however, are useful and instructive.

If the angle PCA in Fig. 2 by denoted be θ, we find by (3) that the surface density σ at P is given by

$$\sigma=\frac{qr}{\pi r_1 r_2}=\frac{q}{2\pi a}\cdot\frac{\sinh a}{\cosh a-\cos\theta}. \quad . \quad . \quad . \quad (11)$$

Similarly, the surface density at any point P' on the B cylinder will be given by

$$\sigma=-\frac{q}{2\pi b}\cdot\frac{\sinh\beta}{\cosh\beta-\cos\varphi}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

where φ is the angle $P'DA$ (Fig. 2).

If we write $\omega=a+\beta$, we have

$$q/(v_1-v_2)=1/(2\omega), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

and

$$\cosh\omega=(c^2-a^2-b^2)/(2ab). \quad . \quad . \quad (14)$$

Let us now consider the case of a cylinder inside a hollow conducting cylinder, the axes of the cylinders being parallel but not necessarily coincident. Let the radius of the inner cylinder be a, the inner radius of the outer cylinder be b,

and let c be the distance between their axes. Then if q be the charge per unit length on the inner cylinder, $-q$ will be the induced charge per unit length on the inner side of the outer cylinder. The electric field between them, and therefore the potential difference between them, will be identically the same as that between two A cylinders whose radii are a and b respectively, the distance between their axes being c. The potentials v_1 and v_2 of these two cylinders when one of the B cylinders has a charge $-q$ per unit length, will be given by

$$v_1=2q\alpha \quad \text{and} \quad v_2=2q\beta\,;$$

and hence

$$\frac{q}{v_1-v_2}=\frac{1}{2(\alpha-\beta)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

This equation therefore gives us the capacity between the inner and the outer cylinder. If we denote $\alpha-\beta$ by ω_1 we easily find that

$$\cosh\omega_1=(a^2+b^2-c^2)/(2ab). \quad . \quad . \quad . \quad . \quad (16)$$

The surface densities at points on the surface of the inner cylinder, and on the inner surface of the outer cylinder, are given by (11) and (12) respectively.

Since q is the charge per unit length of the cylinder, the radius of which is a, it follows at once from (11) that

$$\int_0^\pi \frac{\partial\theta}{\cosh\alpha-\cos\theta}=\frac{\pi}{\sinh\alpha}. \quad . \quad . \quad . \quad . \quad . \quad (17)$$

This equation can easily be verified by the calculus.

The centroid line of the distribution of the electrical charge on the cylinder whose radius is a (Fig. 2) will from symmetry lie in the plane passing through the axes of the two cylinders. If $\bar{x}$ be its distance from O, we have

$$\bar{x}q=2\int_0^\pi \sigma a^2\cos\theta\,\partial\theta=qa\varepsilon^{-\alpha}, \text{ by (17)},$$

and thus

$$\bar{x}=a\varepsilon^{-\alpha}=OA. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

Similarly, the centroid line of the charge on the B cylinder will pass through B.

It will be seen, therefore, that the inverse lines of the cylinders which pass through A and B respectively are the centroid lines of the electrical charges spread over the A and B cylindrical surfaces.

The Electrostatic Attraction Between any Pair of the A or B Cylinders is the Same.

Let us first suppose that the cylinders are external to one another. The attractive force on the A cylinder must remain constant if the electrostatic field surrounding it does not alter in magnitude or direction. The attraction on the A cylinder, therefore, is the same as if the B cylinder were a thin wire through B having a charge $-q$ per unit length. Since the attractions of the cylinders are equal and opposite, the required attraction will be equal to the attraction on this thin wire through B. But the attraction on this thin wire depends only on the number and direction of the lines of induction connected with it. It is independent, therefore, of the size of the A cylinder. Hence the attraction F per unit length between the two cylinders equals that between two thin wires having charges q and $-q$ per unit length respectively, and coincident with their centroid lines. It is therefore given by

$$F=\frac{2q^2}{\lambda\,.\,AB}=\frac{q^2}{\lambda r}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

where λ is the inductivity of the dielectric.

If K be the capacity per unit length between the two cylinders and W the energy stored up in the dielectric, we have

$$W=q^2/(2\mathrm{K})=q^2\omega/\lambda.$$

Hence, $$F=\partial W/\partial c=(q^2/\lambda)(\partial\omega/\partial c).$$

Comparing this equation with (19), we see that

$$\frac{\partial\omega}{\partial c}=\frac{1}{r}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

This equation can also be easily proved directly from the equations (4), (6) and (9), given above.

Similarly, when the sections of the cylinders are both A circles (Fig. 2), so that one of them is inside the other,

$$F=-\frac{q^2}{\lambda}\,\frac{\partial\omega_1}{\partial c}=\frac{q^2}{\lambda r}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

The attraction, therefore, between any pair of cylinders which have the same inverse lines is the same.

The Stream Function.

Let u be the stream function corresponding to the potential function v. If u be zero at L (Fig. 3), we have

$$u=\int_0^\theta \sigma a\partial\theta$$

$$=\frac{q}{2\pi}\int_0^\theta \frac{\sinh a}{\cosh a-\cos\theta}\partial\theta.$$

$$=\frac{q}{\pi}\tan^{-1}\frac{\tan(\theta/2)}{\tanh(a/2)}$$

$$=\frac{q}{2\pi}(\pi-\varphi), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

where θ is the angle PCL and φ is the angle APB.

It follows, as we have shown above, that every line of force such as PQ (Fig. 3) is part of a circle which passes through

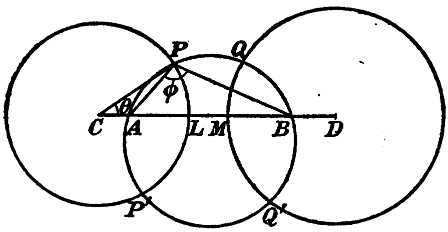


FIG. 3.

Every circle through A and B cuts the circles PLP' and QMQ' at right angles.

A and B. The capacity k_1 of the field per unit length between the portions PP' and QQ' of the cylinders intercepted by any one of these circles is given by

$$k_1=\lambda\,\frac{\frac{q}{2\pi}(\pi-\varphi)+\frac{q}{2\pi}\varphi}{v_1-v_2}=\lambda\frac{\frac{q}{2}}{2q\omega}=\frac{\lambda}{4\omega}. \quad . \quad . \quad . \quad (23)$$

The capacity k_1 is therefore equal to half of the capacity between the cylinders.

If R be the resistance per unit length between the two cylinders supposed of infinite conductivity, and if ρ be the resistivity of the medium between them, we have

$$KR=\rho\lambda/(4\pi),$$

where K is the corresponding capacity. Hence

$$R=\rho\omega/(2\pi),. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

and the resistance R_1 of the medium between PP' and QQ' (Fig. 3) would be given by

$$R_1 = \rho\omega/(4\pi)^* \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

Similarly, if K' denote the thermal conductance between the two cylinders, and k' be the thermal conductivity, we have

$$K' = (4\pi/\lambda)k'K = 2\pi k'/\omega, \quad . \quad . \quad . \quad . \quad . \quad (26)$$

and the thermal conductance between PP' and QQ' (Fig. 3 will be $K'/2$.

Cylinders with Unequal Charges.

Since the sum of the electric charges in a self-contained system must always be zero, it follows that if the sum of the charges on the cylinders be not zero there must be other charged conductors in the system. To fix our ideas we shall suppose that the axis of the cylinder whose radius is a, is also the axis of a very large hollow cylinder, the inner radius of which is c. If v_3 be the potential of this outer cylinder which surrounds the other two, we have

$$v_1 = p_{11}q_1 + p_{12}q_2 + p_{13}q_3,$$
$$v_2 = p_{21}q_1 + p_{22}q_2 + p_{23}q_3,$$

and

$$v_3 = p_{31}q_1 + p_{32}q_2 + p_{33}q_3,$$

where p_{11}, p_{12}, ... are the values of Maxwell's potential coefficients per unit length. If $v_1 = v_2 = v_3$ there can be no charges on the inner cylinders, and thus both q_1 and q_2 are zero. It follows, therefore, that $p_{13} = p_{23} = p_{33}$. Hence

$$v_3 = p_{33}(q_1 + q_2 + q_3).$$

We see that $1/p_{33}$ is the outside capacity per unit length of the hollow cylinder, and by taking this cylinder large enough it can be made as large as ever we please. Hence if we assume that the cylinders are surrounded by a co-axial hollow cylinder at a great distance from them, we can write $p_{13} = p_{23} = p_{33} = 0$.

Our equations simplify to

$$v_1 = p_{11}q_1 + p_{12}q_2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

and

$$v_2 = p_{22}q_2 + p_{12}q_1. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

In the particular case when $q_1 = -q_2 = q$, we have by (7) and (8)

$$v_1 = 2q\alpha, \text{ and } v_2 = -2q\beta.$$

* Cf. G. Carey Foster and O. J. Lodge, "Proc. Phys. Soc.," Vol. I., p. 115, 1875.

Hence, substituting in the simplified equations, we get

$$p_{11}=p_{12}+2\alpha, \quad \text{and} \quad p_{22}=p_{12}+2\beta \quad . \quad .(29)$$

Solving the equations for q_1 and q_2 in terms of v_1 and v_2 we get

$$q_1=k_{11}v_1+k_{12}v_2 \quad \text{and} \quad q_2=k_{22}v_2+k_{12}v_1,$$

where $k_{11}=p_{22}/\Delta$, $k_{22}=p_{11}/\Delta$, $k_{12}=-p_{12}/\Delta$, and $\Delta=p_{11}p_{22}-p_{12}^2$. The coefficients of v_1 and v_2 are called the capacity coefficients, and by considering the case when $q_1=-q_2$, we see that

$$k_{11}=\frac{1}{2\alpha}+\frac{\beta}{\alpha}k_{12}, \quad \text{and} \quad k_{22}=\frac{1}{2\beta}+\frac{\alpha}{\beta}k_{12}. \quad . \quad . \quad (30)$$

We also have

$$k_{11}=\frac{1}{2\alpha+2\beta p_{12}/(2\beta+p_{12})}, \quad \text{and} \quad k_{12}=\frac{-1}{2(\alpha+\beta)+4\alpha\beta/p_{12}}.$$

Since p_{12} is positive, we see that

k_{11} must lie in value between $1/(2\alpha)$ and $1/(2\alpha+2\beta)$,

k_{22} must lie in value between $1/(2\beta)$ and $1/(2\alpha+2\beta)$,

and that $1/2(\alpha+\beta)$ is a superior limit to $-k_{12}$. For instance, if a be very great compared with b, α will be very small compared with β, and thus k_{22} will equal $1/(2\beta)$ very approximately.

From the equations (27), (28) and (29) we deduce that

$$\begin{aligned} v_1-v_2&=2(q_1\alpha-q_2\beta) \\ &=(q_1+q_2)(\alpha-\beta)+(q_1-q_2)(\alpha+\beta). \quad . \quad . \quad (31) \end{aligned}$$

Whatever may be the values of the charges on the conductors this relation always holds. When $q_1+q_2=0$, it gives the capacity between the conductors, and when $q_1=q_2=q$ we have

$$\frac{q}{v_1-v_2}=\frac{1}{2(\alpha-\beta)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (32)$$

Comparing this with equation (15) we see that when the charges on the cylinders are equal, the ratio of the charge to the difference of potentials is the same as for a cylinder of radius a and an enveloping cylinder of inner radius b, provided that the circular cross-sections have the same inverse points in the two cases. The energy stored in the field, however, in the latter case is $(v_1-v_2)^2/4(\alpha-\beta)$, whilst in the former case it is $(v_1^2-v_2^2)/4(\alpha-\beta)$.

When the charge on the B cylinder is zero

$$v_1-v_2=2q_1\alpha. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (33)$$

In this case $q_1/(v_1-v_2)$ is a constant $(1/2a)$ which can be easily found. It is interesting to notice that the value of this constant is the same whichever of the B cylinders is chosen.

A Cylinder Inside a Hollow Cylinder, their Axes being Parallel.

If q_1, q_2, and v_1, v_2 be the charges and potentials of the cylinder (radius a) and sheath (inside radius b) respectively, it is easy to show that

$$q_1=\frac{1}{2(\alpha-\beta)}v_1-\frac{1}{2(\alpha-\beta)}v_2, \quad . \quad . \quad . \quad . \quad (34)$$

and

$$q_2=\left\{C+\frac{1}{2(\alpha-\beta)}\right\}v_2-\frac{1}{2(\alpha-\beta)}v_1, \quad . \quad . \quad (35)$$

where C is the capacity per unit length of the outer cylinder with respect to external bodies.

Hence also,

$$v_1=\left\{\frac{1}{C}+2(\alpha-\beta)\right\}q_1+\frac{1}{C}q_2, \quad . \quad . \quad . \quad . \quad (36)$$

and

$$v_2=\frac{1}{C}q_2+\frac{1}{C}q_1. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (37)$$

Hence when C can be found, we know the complete solution. We see that

$$k_{11}=-k_{12}=\frac{1}{2(\alpha-\beta)}=C_0 \text{ (say)}, \quad . \quad . \quad . \quad (38)$$

and

$$k_{22}=C+C_0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (39)$$

Also

$$p_{11}=\frac{1}{C}+\frac{1}{C_0}, \qquad p_{22}=p_{12}=\frac{1}{C}. \quad . \quad . \quad . \quad (40)$$

If W denote the electrostatic energy,

$$\begin{aligned} W&=\tfrac{1}{2}p_{11}q_1^2+p_{12}q_1q_2+\tfrac{1}{2}p_{22}q_2^2 \\ &=\frac{(q_1+q_2)^2}{2C}+\frac{q_1^2}{2C_0}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (41) \end{aligned}$$

Hence we deduce the following three theorems :—

(a) If q_1+q_2 is constant, W is a minimum when q_1 is zero, and, therefore, by (34) when $v_1=v_2$.

(b) If q_1=a constant, W is a minimum when $q_2=-q_1$, and in this case by (37), $v_2=0$.

(*c*) If q_2=a constant, W is a minimum when

$$q_1 = -\{C_0/(C+C_0)\}q_2, \text{ and then by (36), } v_1=0.$$

We also have
$$W=\tfrac{1}{2}k_{11}v_1^2+k_{12}v_1v_2+\tfrac{1}{2}k_{22}v_2^2$$
$$=\tfrac{1}{2}C_0(v_1-v_2)^2+\tfrac{1}{2}Cv_2^2 \quad . \quad . \quad . \quad . \quad . \quad (42)$$

Hence—

(*a*) If v_1-v_2 is constant, W is a minimum when $v_2=0$, and therefore when $q_2=-q_1$.

(*b*) If v_2 is constant, W is a minimum when $v_1=v_2$. In this case $q_1=0$.

(*c*) If v_1 is constant, W is a minimum when $v_2=\dfrac{C_0}{C+C_0}v_1$.

In this case $q_2=0$. A study of these theorems is instructive.

The force F per unit length between the cylinders when λ is the inductivity of the dielectric is given by

$$F=-\frac{q_1^2}{\lambda r}=-\frac{\lambda(v_1-v_2)^2}{4(\alpha-\beta)^2 r}. \quad . \quad . \quad . \quad . \quad (43)$$

The equilibrium is unstable when the cylinders are co-axial, both when the charge on the inner cylinder or when the potential difference between them is maintained constant. In the former case they move so that the potential energy stored in the field is diminished and in the latter case so that it is increased.

Approximate Values of the Electrostatic Coefficients for Parallel Cylinders.

As a preliminary to finding approximate values for the potential coefficients in equations (27) and (28), let us consider the case of a concentric main, the radius of the inner cylinder being a, and the inner radius of the outer being d. The potential v at a point P between the cylinders distant r from the common axis is given by

$$v=2q_1 \log\frac{d}{r}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (44)$$

where q_1 is the charge per unit length on the inner cylinder. We see that for a given value of q_1 the greater the value of d, the greater will be the value of the coefficients of q_1 in this equation. If d is infinite, v is also infinite. This follows

because the work done in taking unit charge from the infinite cylinder to infinity is infinite.

Let us now consider the case of a very long charged prolate spheroid, the other conductor being a confocal spheroid (practically a sphere) at infinity. If l be the length of the axis of the spheroid and v be the potential at a point P on the equatorial plane at a distance r from the axis, we have

$$v=2q_1 \log \frac{l}{r} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (45)$$

very approximately, where q_1 is the charge on the surface intercepted between any two planes perpendicular to the axis and at unit distance apart.

Formulæ (44) and (45) prove that the actual value of the potential at a point near a charged cylinder even when it is at a great distance away from the ends of the cylinder depends both on the length of the cylinder and on the location of the necessary complementary charge. We notice, however, that the electric force at the point P is to a high degree of approximation independent both of the length and of the position of the complementary charge. Similarly if we have two parallel cylinders at a great distance away from the conductors carrying their complementary charge, we infer that the values of the potential coefficients will vary both with the length of the cylinders and with the position of the other conductors. We are led to infer also that the force per unit length between the cylinders is practically independent of their length and of the location of the complementary charge.

Considering now the case of the concentric main, let us suppose that d is very large and that we have a thin uncharged wire parallel to the axis of the main at a distance c from it, where c is great compared with a but very small compared with d. Since the field is practically undisturbed by the presence of this thin wire, its potential v_2 will be given by

$$v_2=2q_1 \log \frac{d}{c}.$$

Hence we see that

$$p_{12} \doteq 2 \log \frac{d}{c}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (46)$$

and therefore by (29)

$$p_{11}=2\alpha+2 \log \frac{d}{c}; \quad \text{and} \quad p_{22}=2\beta+2 \log \frac{d}{c}. \quad . \quad (47)$$

If we make d infinite, the potential coefficients become infinite, but the capacity coefficients are given by

$$k_{11}=k_{22}=-k_{12}=\frac{1}{2(\alpha+\beta)}=\frac{1}{2\log(c^2/ab)} \quad . \quad . \quad (48)$$

approximately.

The Electrostatic Forces Between the Cylinders.

From (27), (28), (46) and (47) we get

$$v_1=2\left(\alpha+\log\frac{d}{c}\right)q_1+2\log\frac{d}{c}\cdot q_2 \quad . \quad . \quad . \quad (49)$$

and

$$v_2=2\log\frac{d}{c}\cdot q_1+2\left(\beta+\log\frac{d}{c}\right)\cdot q_2 \quad . \quad . \quad . \quad (50)$$

Hence if λ be the inductivity, the electromagnetic energy W stored in the field is given by

$$W\lambda=\frac{1}{2}\left(2\alpha+2\log\frac{d}{c}\right)q_1^2+2\log\frac{d}{c}\cdot q_1q_2+\frac{1}{2}\left(2\beta+2\log\frac{d}{c}\right)q_2^2 \quad (51)$$

$$=\alpha q_1^2+\beta q_2^2+\log\frac{d}{c}\cdot(q_1+q_2)^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (52)$$

Hence

$$F=\frac{\partial W}{\partial c}$$

$$=\frac{1}{\lambda}\left\{q_1^2\frac{\sinh\alpha\cosh\beta}{r\sinh\omega}+q_2^2\frac{\cosh\alpha\sinh\beta}{r\sinh\omega}-\frac{(q_1+q_2)^2}{c}\right\} \quad . \quad (53)$$

$$=\frac{1}{2\lambda c^2 r}\{q_1^2(c^2+b^2-a^2-2cr)+q_2^2(c^2+a^2-b^2-2cr)-4q_1q_2cr \quad (54)$$

By Kelvin's theorem (l.c. *ante*, p. 150) the conductor must move so as to diminish the electrostatic energy W. Hence when F is positive, that is when W increases with c the force is attractive, and when F is negative it is repulsive. We may write equation (54) in the form

$$F=\frac{1}{2\lambda c^2 r}(Aq_1-Bq_2)(Aq_1-Cq_2), \quad . \quad . \quad . \quad (55)$$

where
$$A^2=c^2+b^2-a^2-2cr,$$
$$B=(2cr+N)/A, \quad C=(2cr-N)/A,$$
and
$$N^2=(b^2-a^2)^2+c^3(4r-c).$$

We see that when q_1 and q_2 are of opposite signs, F is always positive and, therefore, the force is always attractive. When, however, q_1 and q_2 have the same sign F is attractive, zero or repulsive according as q_1/q_2 is greater than B/A or less than C/A, equals either of these quantities or lies in value between them.

Particular cases are of interest. If $q_1=-q_2=q$ (54) reduces to (19), which is exactly true. If, in addition $a=b$, we have

$$F=\frac{2q^2}{\lambda(c^2-4a^2)^{\frac{1}{2}}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (56)$$

When $q_1=q_2=q$, the formula reduces to

$$F=-\frac{(4r-c)q^2}{\lambda cr}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (57)$$

In the particular case when the B cylinder is so thin that we may write $b=0$, and therefore $2cr=c^2-a^2$, we get

$$F=\frac{2q_2\{a^2q_2-(c^2-a^2)q_1\}}{c(c^2-a^2)}. \quad . \quad . \quad . \quad . \quad (58)$$

If q_2 is zero, F vanishes. When q_1 is zero the attractive force is given by

$$F=\frac{2a^2q_2^2}{c(c^2-a^2)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (59)$$

We see from (58) that when q_1 and q_2 have the same sign the force is attractive, when q_2/q_1 is greater than $(c^2-a^2)/a^2$;

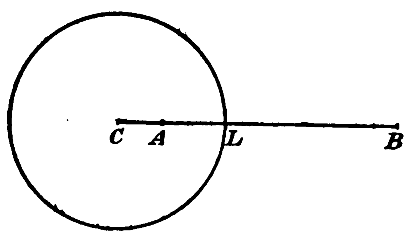


FIG. 4.

$CA.\ CB=CL^2$.

The wire which is the image of the wire through B passes through A.

it vanishes when q_2/q_1 has this value and it is repulsive, when q_2/q_1 is less than $(c^2-a^2)/a^2$.

A direct geometrical proof of (58) can easily be given by the method of images as follows :—

Let C and B (Fig. 4) be the centres of the cross-sections of the cylinder and wire respectively. If the charge per unit

length on the wire be q_2, its image in the cylinder will be a wire through A having a charge $-q_2$ per unit length, where $CA \,.\, CB=a^2$. Hence if the total charge on the cylinder be q_1 per unit length we can replace the cylinder by two parallel wires through C and A respectively, which have charges q_1+q_2 and $-q_2$ per unit length respectively. Hence we see at once that the force F is given by

$$F=\frac{2q_2(q_1+q_2)}{c}-\frac{2q_2{}^2}{c-a^2/c}$$

$$=\frac{2q_2\{a^2q_2-(c^2-a^2)q_1\}}{c(c^2-a^2)},$$

which is equation (58).

High Frequency Currents.

We know that at very high frequencies the currents distribute themselves over the surface of the cylinders in such a way that there are no magnetic lines of force produced in the metal. We thus see that when the currents are equal to $+I$ and $-I$ the current density i per unit of the circumference of the A cylinder is given by

$$i=\frac{I}{2\pi a}\cdot\frac{\sinh \alpha}{\cosh \alpha-\cos \theta}. \quad . \quad . \quad . \quad . \quad . \quad (60)$$

See Fig. 2 and compare with equation (11).

Similarly for the B cylinder,

$$i=-\frac{I}{2\pi b}\cdot\frac{\sinh \beta}{\cosh \beta-\cos \theta}. \quad . \quad . \quad . \quad . \quad (61)$$

Since $l/r=1-e\cos\theta$ is the equation to an ellipse referred to the focus as origin and the major axis as initial line, l being the semi latus rectum and e the eccentricity, we see that the length Cp of the radius vector of the ellipse MpM' in Fig. 5 gives the current density i at the point P on the cylinder A when carrying high frequency currents, the eccentricity of the ellipse being sech α and its major axis $(I/\pi a)$ coth α.

The ratio of the greatest to the least current density on the A cylinder equals CM/CM', which equals $\coth^2(\alpha/2)$.

Let us now consider the case when a potential difference e_1 per unit length is applied to the A cylinder and a P.D. e_2 per unit length to the B cylinder. If I_1 and I_2 are the currents

produced on the cylinders, then, since with high frequencies the resistance terms can be neglected, our equations are

$$e_1 = L_{11}\frac{\partial I_1}{\partial t} + L_{12}\frac{\partial I_2}{\partial t} \quad . \quad . \quad . \quad . \quad . \quad (62)$$

and

$$e_2 = L_{22}\frac{\partial I_2}{\partial t} + L_{12}\frac{\partial I_1}{\partial t}, \quad . \quad . \quad . \quad . \quad . \quad (63)$$

where L_{11}, L_{22} and L_{12} are the inductance coefficients with high frequency currents.

Integrating these equations we get

$$\Phi_1 = L_{11}I_1 + L_{12}I_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (64)$$

and

$$\Phi_2 = L_{22}I_2 + L_{12}I_1, \quad . \quad . \quad . \quad . \quad . \quad . \quad (65)$$

where Φ_1 and Φ_2 are the linkages of the lines of induction with the currents in the A and B cylinders respectively.

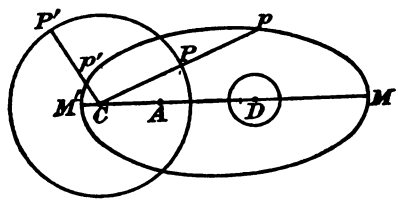


FIG. 5.

The current density at the point P on the cylinder equals Cp, where C is the focus of the ellipse whose major axis is in the same line as $C\,D$, and for which $C\,M = (I/2\pi a)\coth(\alpha/2)$; $C\,M' = (I/2\pi a)\tanh(\alpha/2)$ and the eccentricity $= \operatorname{sech}\alpha = \dfrac{2ca}{c^2 + a^2 - b^2}$.

Let us first suppose that the cylinder A is inside the cylinder B. Since the currents are so distributed that the resultant magnetic force inside the metal of either conductor is zero we see by comparing (36) and (37) with (64) and (65) that

$$L_{11} = 1/C + 2(\alpha - \beta), \quad \text{and} \quad L_{12} = 1/C = L_{22}. \quad (66)$$

If $I_1 = -I_2$ the self inductance L, per unit length, of the circuit formed by the two cylinders is given by

$$L = L_{11} + L_{22} - 2L_{12} = 2(\alpha - \beta). \quad . \quad . \quad . \quad . \quad (67)$$

When the circuits are in parallel $e_1 = e_2$, and hence

$$I_1 = \frac{L_{22} - L_{12}}{L_{11} + L_{22} - 2L_{12}}(I_1 + I_2) = 0 \quad . \quad . \quad . \quad . \quad (68)$$

since $L_{22} - L_{12} = 0$. Thus all the current flows on the outside of the outer conductor.

If the outer cylinder is insulated so that $e_2=0$, we have $L_{22}I_2+L_{12}I_1=0$, and so $I_1=-I_2$. The induced current on the outer cylinder is thus equal and opposite to the current on the inner cylinder. Comparing also with the analogous electrostatic problem, we see that it is on the inside of the outer cylinder.

If the inner cylinder is insulated so that $e_1=0$, we have

$$I_1=-\frac{L_{12}}{L_{11}}I_2=-\frac{C_0}{C+C_0}I_2.$$

On the inner surface of the outer cylinder, therefore, there must flow a current $C_0I_2/(C+C_0)$, and on the outer surface a current $CI_2/(C+C_0)$. When this occurs the electromagnetic energy

$$\tfrac{1}{2}L_{11}I_1^2+L_{12}I_1I_2+\tfrac{1}{2}L_{22}I_2^2 \quad . \quad . \quad . \quad . \quad (69)$$

has a minimum value, since I_2 is constant.

Since L_{12} and L_{22} are independent of c, the distance between the axes of the cylinders, the force F per unit length between them is given by

$$F=\tfrac{1}{2}\frac{\partial L_{11}}{\partial C}I_1^2=\tfrac{1}{2}\frac{\partial}{\partial C}\left\{\frac{1}{C}+2(\alpha-\beta)\right\}I_1^2=-\frac{I_1^2}{r} \quad . \quad . \quad (70)$$

the equilibrium being stable when the cylinders are co-axial.

When the cylinders are solid and parallel to one another we have

$$e_1=\left(2\alpha+2\log\frac{d}{c}\right)\frac{\partial I_1}{\partial t}+2\log\frac{d}{c}\cdot\frac{\partial I_2}{\partial t} \quad . \quad . \quad . \quad (71)$$

and

$$e_2=\left(2\beta+2\log\frac{d}{c}\right)\frac{\partial I_2}{\partial t}+2\log\frac{d}{c}\cdot\frac{\partial I_1}{\partial t} \quad . \quad . \quad . \quad (72)$$

approximately. The greater the values of d/c, c/a and c/b the more accurate will be the equations. Hence

$$L_{11}=2\alpha+2\log\frac{d}{c};\quad L_{22}=2\beta+2\log\frac{d}{c};\quad L_{12}=2\log\frac{d}{c}.$$

Thus the self inductance L per unit length of the circuit formed by the two cylinders in series will be given by

$$L=L_{11}+L_{22}-2L_{12}=2(\alpha+\beta) \quad . \quad . \quad . \quad . \quad (73),$$

an equation which is exactly true at all distances apart.

Comparing (10) and (73) we see that $LK=1$.

If W be the electromagnetic energy stored in the field at any instant, we have

$$W=\alpha I_1^2+\beta I_2^2+\log\frac{d}{c}\cdot(I_1+I_2)^2 \quad . \quad . \quad . \quad (74)$$

When the cylinders are in parallel we get

$$I_1=\frac{\beta}{\alpha+\beta}(I_1+I_2), \quad . \quad . \quad . \quad . \quad . \quad (75)$$

or

$$I_2=\frac{\alpha}{\alpha+\beta}(I_1+I_2).$$

At every instant, therefore, the ratio I_1/I_2 equals β/α. Since $\beta/\alpha=\sinh^{-1}(r/b)/\sinh^{-1}(r/a)$, we see that if b is greater than a, I_1/I_2 is less than unity. Hence the smaller conductor carries the smaller current. The density of the current on the smaller conductor is also less than on the larger conductor. It is to be noticed that (75) is the condition that (74) has a minimum value when I_1+I_2 is a constant.

If F be the instantaneous value of the E.M.F. acting on the cylinders per unit length, we have

$$F=\frac{\partial W}{\partial c}=I_1^2\frac{\sinh\alpha\cosh\beta}{r\sinh\omega}+I_2^2\frac{\cosh\alpha\sinh\beta}{r\sinh\omega}-\frac{(I_1+I_2)^2}{c} \qquad (76)$$

$$=\frac{1}{2c^2r}\{I_1^2(c^2+b^2-a^2-2cr)+I_2^2(c^2+a^2-b^2-2cr)-4crI_1I_2\} \qquad (77)$$

$$=\frac{1}{2c^2r}(AI_1-BI_2)(AI_1-CI_2) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (78)$$

where A, B and C have the same values as in (55).

Now by Kelvin's theorem* if I_1 and I_2 are maintained constant the conductors move so as to increase the potential energy. Since W increases with c when F is positive, it follows that when F is positive the force is repulsive, and when negative it is attractive. We deduce from (78) that when I_1 and I_2 are of opposite signs the force is always repulsive. When, however, the currents I_1 and I_2 are flowing in the same direction the force is attractive when I_1/I_2 lies in value between B/A and C/A. When the ratio of these currents equals either of these limiting values the force vanishes, and when it lies outside those limits the force is repulsive.

In practice we are concerned with the average value of the force F taken over a whole period. If φ be the phase difference between the currents I_1 and I_2, if F' denote the average value of F, and I_1' and I_2' be the effective values of the currents, we have by (78)

$$F'=\frac{1}{2c^2r}\{A^2I_1'^2+B^2I_2'^2-A(B+C)I_1'I_2'\cos\varphi\}. \quad . \quad (79)$$

* Russell's "Alternating Currents," Vol. I., p. 41.

When $\cos\varphi=1$ or -1, the equation is practically identical with (78). In any case, however, it can easily be put into factors and discussed in a similar way.

In the particular case when $I_1=I_2=I$, we have $\cos\varphi=1$, and

$$F'=-\frac{I^2}{cr}(4r-c), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (80)$$

the force being always attractive.

When $I_1=-I_2=I$, $\cos\varphi=-1$, and hence

$$F'=\frac{I^2}{r}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (81)$$

the force being repulsive. It is to be noticed that (80) is only an approximate formula, while (81) is an exact formula.

When the cylinder B is a very fine wire, formulæ (76) to (78) are extremely accurate. Putting $b=0$ and $2cr=c^2-a^2$, in 77 we get

$$F=\frac{2I_2\{a^2I_2-(c^2-a^2)I_1\}}{C(c^2-a^2)}. \quad . \quad . \quad . \quad . \quad . \quad (82)$$

It can be readily shown by the method of images that this formula is exact. We have also

$$F'=\frac{2I_2'\{a^2I_2'-(c^2-a^2)I_1'\cos\varphi\}}{C(c^2-a^2)}. \quad . \quad . \quad (83)$$

We see that when I_2'/I_1' is greater than $(c^2-a^2)\cos\varphi/a^2$ the force is repulsive. When this ratio equals $(c^2-a^2)\cos\varphi/a^2$ the force vanishes, and when it is less than this value it is attractive.

It follows also from (83) that when the distance between the wire and the axis of the cylinder is less than

$$a\,\{(I_1'\cos\varphi+I_2')/I_1'\cos\varphi\}^{\frac{1}{2}}$$

the force is repulsive, when it is equal to it the force vanishes, and when it is greater than this value it is attractive.

Hence when

$$c=a\,\{(I_1'\cos\varphi+I_2')/I_1'\cos\varphi\}^{\frac{1}{2}} \quad . \quad . \quad . \quad (84)$$

the wire is in a position of stable equilibrium.

In conclusion, physicists should bear in mind that the potential and capacity coefficients of conductors have perfectly determinate values. Even in simple cases values are difficult to find by calculation, but in every case they can be

found accurately by experiment. If we compare (27) and (28) with (64) and (65) we see at once that $L_{11}=p_{11}$, $L_{12}=p_{12}$ and $L_{22}=p_{22}$.* It follows that the inductance coefficients of conductors for high frequency alternating currents can be found very simply by determining experimentally the potential coefficients of the conductors for electrostatic charges.

ABSTRACT.

Many problems in connection with parallel cylindrical conductors occur in practical electrical work. The formulæ for the capacity between the conductors and for the effective inductance are well known, but the values of the capacity and potential coefficients and of the inductance coefficients have not yet been determined. It is shown that for the case of a cylinder inside a cylindrical tube their values can in all cases be easily computed. When the cylinders are external to one another, it is proved that the three capacity coefficients are connected by two very simple relations. Limiting values between which these coefficients must lie are found, and methods of obtaining closely approximate values in special cases are given. Whatever the charges on the cylinders may be, provided that the other conductors of the system are remote, the mutual force between them can be calculated to high accuracy when their distance apart is great or when the radius of one is small compared with that of the other.

Practically identical formulæ enable us to find the current-density and the inductance coefficients with high-frequency currents, both for a cylinder inside a cylindrical tube and for two parallel cylinders. In the latter case it is shown that when the phase difference between the currents is less than 90 deg. the mechanical force between the cylinders is repulsive when they are close together and attractive when they are far apart. At a definite distance apart, therefore, the cylinders when carrying high-frequency currents are in stable equilibrium. Since the potential coefficients can always be determined experimentally, it follows that the inductance coefficients for high-frequency currents which are equal to them are also found by the same experiments.

DISCUSSION.

Dr. D. Owen said that in the first paragraph of the section on High Frequency Currents, the author said "We thus see—&c." He did not quite know the justification for this.

Mr. F. J. W. Whipple said it surprised him that the solutions to these problems had not all been already worked out. It appeared clear to him that one could write down the solutions in θ functions without much trouble. By so doing many difficulties might be got over since these functions were tabulated. Dr. Russell handled infinities rather familiarly, and he was not certain of the reliability of some of the solutions. Then he obtained a result for a thin wire and assumed it to hold for a thick one. One did not know how far the assumption was justified.

Dr. Russell, in reply, said the object of the Paper was to obtain approximate solutions to clear the ground for a complete discussion of the problem. Dr. Owen's point was explained in one of Kelvin's works, to which he would send him the reference.

* Cf. *l.c. ante*, Vol. I., p. 201.

XII. *Temperature Coefficient of Tensile Strength of Water.*

By S. SKINNER, *M.A., and* R. W. BURFITT, *B.Sc.*

RECEIVED DECEMBER 9, 1918.

IN a Paper on the effect of temperature on the hissing of water when flowing through a constricted tube (Roy. Soc., " Proceeding A," Vol. XCI., 1915, p. 481), Skinner and Entwistle described the measurements which indicated that the tensile strength of water became zero at a temperature of about 320°C., a temperature approaching the critical point of water. The conclusion was drawn that the tensile strength of water diminished with temperature to the critical point. To make the law general, it is necessary to examine the behaviour of other liquids, but the original form of the apparatus was quite inappropriate, since the water at a high pressure was drawn directly from the water supply of the building. It was with a view of designing a small apparatus in which other liquids could be tried that the following experiments were commenced.

Meanwhile Sir Joseph Larmor (" Proc." Lond. Maths. Soc., 1916, p. 191), on the assumption that the van der Waals form of equation holds in the liquid state, arrived at the theoretical conclusion that the negative pressure could subsist only at absolute temperatures below 27/32 of the critical point of the substance. For water the critical point is 365°C.; thus in this substance, internal tension could persist to 538°Abs., or 265°C. This conclusion was of interest, since the experiments quoted above had shown that the tensile strength probably disappeared at about 320°C., and from what follows, this result appears to be more nearly 245°C., which is in agreement with the theory.

In the new experiments the tube which contained the water consisted of two vertical wide-bore arms, length 22 cm., diameter 3 cm., connected by a tube, length 12 cm., the central portion of which was a capillary of length 2·5 cm., and bore 1·195 mm. diameter. The arrangement of the apparatus is shown in the diagram. We shall now describe the method of making the experiments.

Air was pumped into a cylinder *R*, of some 6 litres capacity, which was kept at a constant temperature by a water jacket,

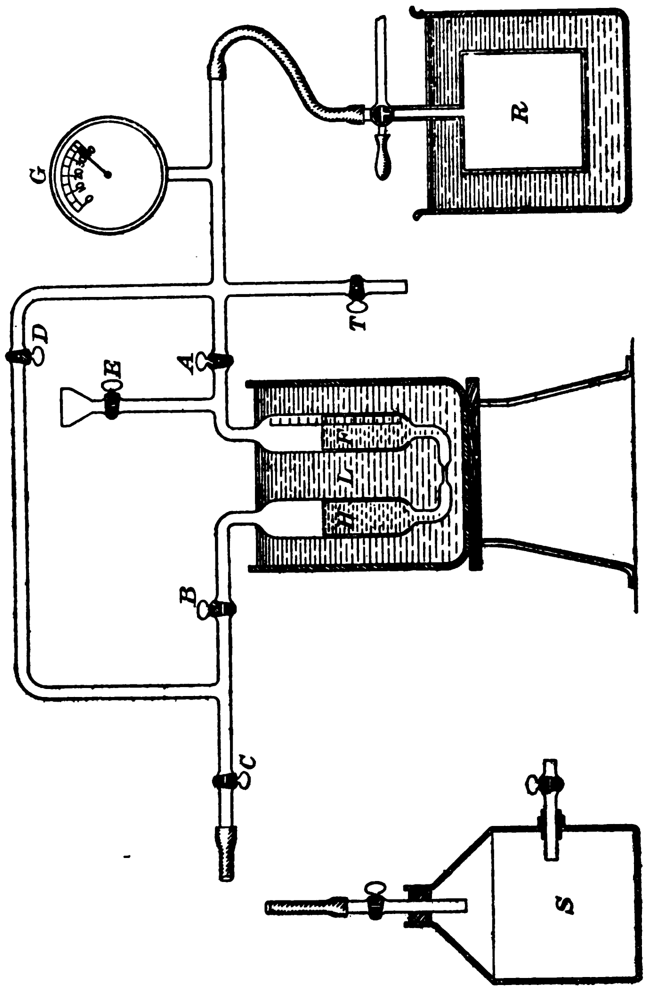


FIG. 1.—DIAGRAM OF APPARATUS.

Fig. 1. From this reservoir, the air pressure could be exerted along different paths by the adjustment of taps. A gauge, G, recorded the pressure of the air. The taps A, B being turned on, and the tap C being open to the air, and D, E and T being shut, the air forced the liquid from limb F of experimental tube through constriction into limb H. When taps C, A and T were shut and E open, the air could be used to drive the liquid back to its initial position. By trial it was possible to make a nice adjustment of the pressure of the air in R, so that the speed for rupture could be instantly given to the liquid. The rupture of the liquid was generally judged by the sound, although the appearance of the liquid as regards cloudiness produced by little drops was also noted. Along the arm F a vertical scale 15 cm. long was fixed. A stop-watch reading to one-fifth of a second was used to obtain the time required for the liquid to descend through a particular range, and thus the relative speeds in the constriction could be deduced.

The air reservoir S, with its two taps, could be attached when a back pressure was required. This may be necessary when the temperature of the liquid under examination is near the boiling point. Moreover, the use of a back pressure enables observations to be made at temperatures beyond the normal boiling point.

It will be observed that in this form of apparatus the same sample of liquid is used over and over again. In this, the experiments differ from the former, in which fresh liquid was used for each observation. Moreover, the quantity of liquid required is only small, which is necessary if the method is to be used for liquids other than water.

A large number of observations, with the tap C being open to the air, were made up to a temperature near 90°C., and between 90° and 100°C. with the aid of a back pressure in the reservoir S. These observations are shown in a diagram, Fig. 2, and a straight line drawn through them cuts the axis at a temperature near 245°C.

The actual observations for velocity and temperature are recorded in the table and also columns are given, one containing the pressure indicated by the gauge, and another giving the value of the coefficient of velocity when the velocity at 250°C. is assumed to be zero.

This result confirms the general conclusion obtained in the paper of Skinner and Entwistle, and is in agreement with

Sir Joseph Larmor's theoretical views. If V is the velocity of the current at the constriction at a temperature t°C., and θ°C. the critical temperature of water and C a constant, then

$$V = C\left\{\frac{27}{32}(\theta+273)-(t+273)\right\}$$

The experiments show that this formula is true for temperatures between 0°C. and 100°C.

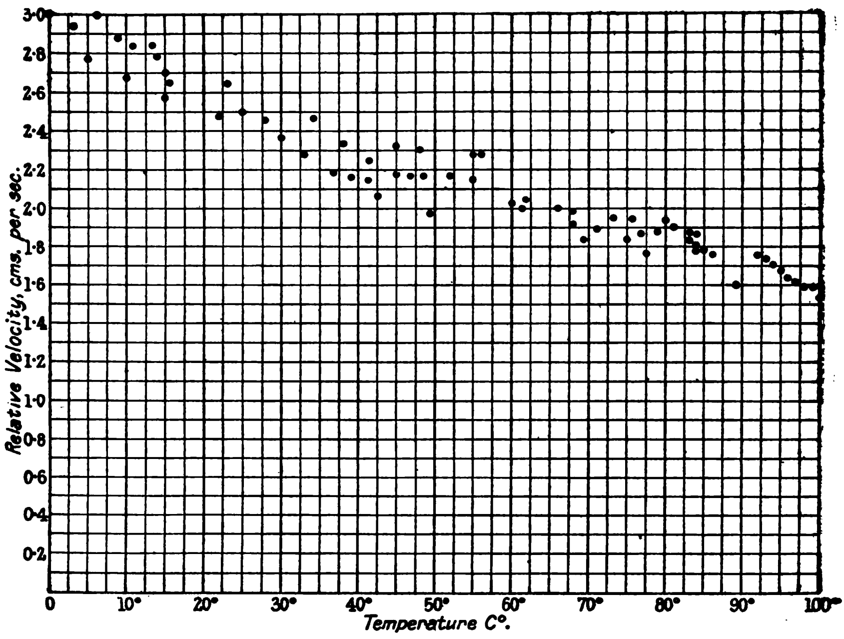


FIG. 2.—GRAPH OF TEMPERATURE VELOCITY, OCTOBER, 1917–18.

We are now making a much stronger apparatus for the study of oils, and some preliminary experiments appear to indicate that a similar law holds for them.

TABLE OF RESULTS.

Date.	*t.* Temperature.	*a.* Proportional Velocity.	*b.* Pressure in lb. per sq. in.	$\frac{a}{250-t}$
	Deg. C.	Cms.	Lbs.	
Oct. 29, 1917	15·5	2·65	29·5	0·01133
	22·0	2·48	24·5	0·01087
	33·0	2·28	20·0	0·01051
	37·0	2·16	19·0	0·01028
	39·0	2·19	18·0	0·01023
Nov. 5, 1917	30·0	2·37	21·5	0·01076
	38·0	2·33	19·5	0·01099
	41·5	2·24	18·5	0·01072
	45·0	2·18	17·5	0·01064
	48·5	2·17	17·5	0·01076
	52·0	2·16	17·0	0·01091
Nov. 10, 1917	55·0	2·15	16·0	0·01103
	61·5	2·00	15·5	0·01060
	69·5	1·83	15·0	0·01014
	75·5	1·94	14·8	0·01112
	81·0	1·89	14·4	0·01119
	83·0	1·86	14·2	0·01114
Nov. 12, 1917	66·0	2·03	16·4	0·01069
	68·0	1·91	15·2	0·01049
	77·0	1·87	14·5	0·01081
	83·0	1·82	13·5	0·01090
Nov. 15, 1917	73·0	1·94	15·1	0·01096
	79·0	1·88	14·0	0·01100
	85·0	1·78	12·5	0·01078
Nov. 28, 1917	75·0	1·83	15·0	0·01046
	84·0	1·80	13·0	0·01100
	86·0	1·75	12·5	0·01067
	62·0	2·04	16·8	0·01085
	47·0	2·17	18·5	0 01069
Nov. 29, 1917	66·0	2·00	15·5	0·01086
	80·0	1·92	14·8	0·01129
	68·0	1·98	15·2	0·01087
	56·0	2·28	16·4	0·01175
	48·0	2·30	17·4	0·01139
	45·0	2·31	18·2	0·01127
Dec. 14, 1917	13·5	2·84	36·0	0·01200
	34·5	2·46	20·0	0·01090
	41·5	2·14	17·5	0·01026
	42·5	2·06	17·5	0·00995
	49·5	1·97	17·0	0·00984
	55·0	2·28	15·9	0·01169
	71·0	1·89	16·0	0·01055
	84·0	1·85	13·0	0·01115
	89·0	1·60	12·2	0·00996
Jan. 11, 1918	0·0	3·00	42·0	0·01200
	5·0	2·77	38·0	0·01131
	10·0	2·68	36·0	0·01116
	15·0	2·58	33·0	0·01097
	23·0	2·64	29·0	0·01163
	25·0	2·50	28·0	0·01106
	28·0	2·45	26·0	0·01104

TABLE OF RESULTS.—*Continued.*

Date.	t. Temperature.	a. Proportional Velocity.	b. Pressure in lb. per sq. in.	$\frac{a}{250-t}$
Jan. 26, 1918	0·0	3·00	42·0	0·01200
	3·0	2·94	38·0	0·01191
	9·0	2·88	36·5	0·01195
	11·0	2·83	36·0	0·01184
	14·0	2·78	34·5	0·01178
	15·0	2·70	33·0	0·01149
Feb. 23, 1918	92·0	1·75	15·0	0·01108
	93·0	1·72	15·0	0·01095
Mar. 9, 1918	94·0	1·70	14·0	0·01090
	95·0	1·66	13·0	0·01071
	96·0	1·63	12·0	0·01064
	97·0	1·61	11·0	0·01052
	98·0	1·59	8·0–9·0	0·01047
	99·0	1·59	8·0–9·0	0·01053
	100·0	1·53	8·0–9·0	0·01020

South Western Polytechnic Institute, Chelsea.

ABSTRACT.

The liquid is forced under pressure through a capillary constriction between two limbs of a U-tube. By trial the pressure is adjusted until the speed in the capillary is sufficient to produce rupture. This is judged by the sound and also the appearance. The whole U-tube is immersed in a bath, the temperature of which can be varied. Actual observations of rupture, velocity and temperature are recorded up to about 100°C., from which it is deduced that the tensile strength becomes zero in the neighbourhood of 245°C., which is in agreement with theory.

DISCUSSION.

Dr. VINCENT asked what arrangements were made to get the liquid back into the virgin state after it had been churned up by forcing through the capillary.

Dr. BRYAN asked how dissolved gas affected the results. In experiments by Worthington and others the water required to be very carefully boiled before it showed any tensile strength at all.

Prof. LEES also commented on the fact that the liquid experimented on here contained dissolved gas. In Worthington's experiments with gas-free water the tensile stress was about 2 atmospheres, as far as he remembered. This seemed of a different order from that obtained by the authors.

Mr. F. J. W. WHIPPLE asked where the tensile strength came into the formula. The diagram seemed to connect velocity and temperature only.

Mr. BURFITT, in reply to Dr. Vincent, said the liquid was always allowed to stand until all cloudiness had disappeared, and any dissolved gas was in equilibrium. There was no doubt the presence of dissolved gas promoted rupture. Worthington's method was static, and there was no danger of air getting in if the liquid was initially air free. In the present method it was impossible to prevent some air becoming dissolved. With reference to Mr. Whipple's comments, the tables are not primarily intended to give actual values of the tensile stress, but to give its variation with temperature. If desired, the value of C in the theoretical formula can be calculated from the figures given.

XIII. *Vector Diagrams of Some Oscillatory Circuits Used with Thermionic Tubes.* *By* W. H. Eccles, *D.Sc.*

Received January 12, 1919.

The circuits used in generating electrical oscillations by aid of three-electrode thermionic tubes or relays are now well known, and need not be fully described here; but the general principles of operation may be briefly summarised.

The practical problem is how to sustain the electrical oscillations in a given oscillatory circuit by aid of a rapidly acting relay such as an ionic tube. Evidently from analogy with the balance wheel of a watch and its escapement this can be done by applying properly timed E.M.F.'s. Any relay can do this if the energy taken from the oscillator to operate the relay is small compared with the amount given to the oscillator by the action of the relay. To be more precise, the energy

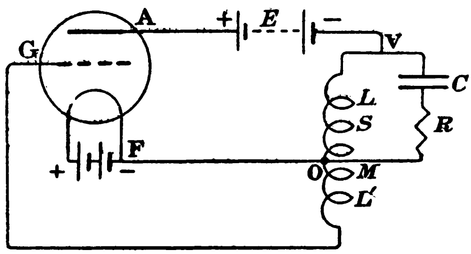


Fig. 1.

given to the oscillator must be greater than that taken to operate the relay by the amount expended in the oscillator as useful work and as waste.

One of the simplest ways of associating an oscillatory circuit with an ionic tube is given in Fig. 1. Here the oscillator is made up of an inductance coil L and a condenser C connected in parallel. The common terminals of coil and condenser are connected to the filament and the anode of the tube, a battery of high voltage, E, being inserted in either lead to the tube. The battery causes electrons to flow from the filament to the plate, and if nothing else is done a steady current appears to flow into the tube at its anode and out at the filament and through the inductance coil L. This flow is necessary in order to set up the space charge upon which the grid of the tube acts when the tube is used as a relay.

In the mode of connection shown in the Figure the grid is acted upon by E.M.F. induced in the grid coil L' by means of its mutual inductance M with the oscillator coil. When the grid is made positive relative to the filament the current in the plate circuit increases, and when it is made negative the current decreases. The circuit FL'GF is called the control circuit, and the circuit FOVEAF is sometimes called the repeat circuit.

If we suppose an oscillation started in the circuit L, C an oscillatory E.M.F. is induced in L' and applied to G. Let its value be e_g; then by the properties of the tube this is transferred to the repeat circuit as an E.M.F. ge_g, where g may be called the voltage ratio of the tube and is of order about 10 in many tubes. If the mutual inductance is of the right sign this E.M.F. acts at the oscillator terminals OV in the right sense to assist the oscillatory current running at the instant, but if it is of the wrong sign the control E.M.F. tends to stop the oscillatory current. The sign of the mutual inductance is altered by reversing one of the coils. When the mutual inductance is of the right sign and is great enough, and if certain other conditions to be discussed are satisfied, the oscillation in L, C will be maintained, the energy expended in resistances R and S being supplied by the battery E.

I. Langmuir* gave the empirical equation

$$I=A(V_a+gV_g)^{3/2},$$

to represent the total current I through the tube from plate to filament when the voltage between plate and filament is V_a and that between grid and filament V_g. M. Latour† and G. Vallauri‡ introduced approximate equations for discussing small changes of voltage and current, and these may be written in the form

$$I_0+i=h_0+h_aV_a+h_gV_g,$$

where I_0 is the steady part of the current through the tube, i the variable part of the current, V_a, V_g the voltages (relative to the filament) applied to anode and grid, and h_0, h_a, h_g are constants of the tube having fairly definite values at a fixed temperature of filament. Usually V_a includes a constant and a variable portion, say E and e_a, and a similar statement holds

* I. Langmuir, "Gen. El. Rev.," XVIII., pp. 327-339, May, 1915.
† M. Latour, "Electrician," LXXVIII., pp. 280-282, December, 1916.
‡ G. Vallauri, "L Elettrotecnica," January 25–February 5, 1917.

for V_g, and then if we ignore the unvarying parts of the current and the voltage we obtain the equation

$$\begin{aligned} i &= h_a e_a + h_g e_g, \\ &= h_a(e_a + g e_g). \end{aligned}$$

The quantities h_a and h_g are of the nature of conductances.

In the mode of connecting oscillator to tube shown in Fig. 1 the anode current when it is steady all passes through the inductance coil of the oscillator ; but when the anode current has a variable part this divides between the inductances and

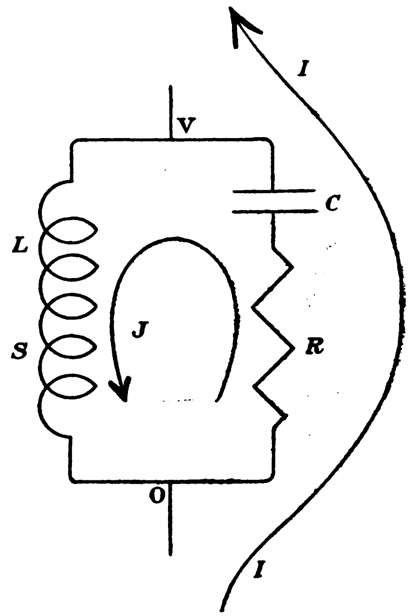


FIG. 2

the condenser branches. This division may be represented by the cyclic notation as indicated in Fig. 2. We shall at once assume the variable current to be of sine form, and shall therefore write

$$\begin{aligned} i &= I \sin \omega t, \\ j &= J \sin (\omega t + \theta). \end{aligned}$$

On this assumption we shall draw the vector or crank diagram of the circuit. In Fig. 3 OU is taken in any direction on the paper to represent to scale the fall of potential along the

resistance R due to the current I alone, that is $\mathrm{OU}=RI$. In accordance with the usual convention the changing values of the P.D. at the ends of the resistance R are given by rotating the vector OU counter-clockwise and taking its projection upon a fixed line in the plane of rotation. In the same way UD is drawn to represent the fall of potential through the condenser due to the current I; it is well known that this will be of magnitude $I/C\omega$ and 90 deg. behind the vector OU. But the circulating current J also traverses R and C; let the consequent potential fall in C be represented by $\mathrm{DQ}=J/C\omega$, and that in R by $\mathrm{QV}=RJ$. The current J flows also in the inductance L and resistance S. The fall of potential in S must be in phase with that due to the same current in R, and

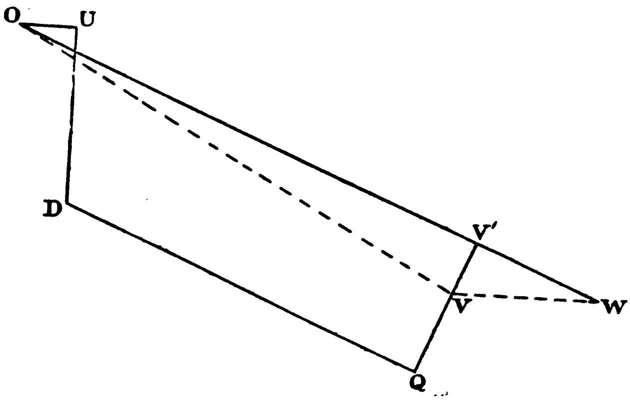


FIG. 3.

that in L must be in opposite phase to that in C and therefore VV′, the prolongation of QV is made equal to SJ and $\mathrm{V'O}=L\omega J$. This last vector is made to end at O in order to fulfil the condition that in going round the closed circuit C, R, L, S the total fall of potential, reckoned with due regard to the phase in each element, must be zero since there is no source of E.M.F. in the closed circuit.

The points O and V on the crank diagram of Fig. 3 correspond to the points O and V in Fig. 2, and in fact the line joining O to V in the crank diagram represents the magnitude and phase of the fall of potential across the oscillator due to the sine current of amplitude I traversing it. Since the applied E.M.F. in the anode circuit of the tube is an unvarying one E,

the voltage actually applied to the plate is alternately less and greater than E to an extent determined by the position of the representative vector OV. Moreover, when the alternating current i flows through the tube the fall of potential between the plate and the filament inside the tube is, we have seen, equal to I/h_a in amplitude and, of course, in phase with the vector RI or OU. This new vector is drawn in Fig. 3 at VW, so as to add geometrically to OV. The result is OW. It is placed in the same direction as OV′ for a reason now to be explained.

It is plain that in order to maintain steady the oscillatory currents represented by the crank diagram the ionic relay must supply the alternating E.M.F. required to drive the current i through the oscillator and the tube, that is, it must supply the E.M.F. represented by OW. Hence the voltage applied to the grid must be an alternating voltage in phase with OW and of $1/g$th the magnitude. But in the circuit of Fig. 1 the grid voltage is supplied by induction from the coil L. We shall assume that the current flowing to the grid under this induced voltage is negligible—as is almost always permissible —and then the reaction of the induced current upon the primary current is also negligible. Then the voltage induced in the grid circuit is of amplitude $M\omega J$ and is in phase with the potential fall $L\omega J$ in the coil L, or in exact opposition, according to the sign of M. In Fig. 1 the mutual inductance is such that when the fall of potential is from O to V the induced voltage makes the grid positive with respect to the filament. We therefore measure along OV′ a length equal to M/L times OV′ in order to represent the E.M.F. applied to the grid. As already seen, the action of the tube is equivalent to the application in the plate circuit of a voltage g times that applied to the grid. But the E.M.F. required in the plate circuit is measured by OW in the crank diagram. Hence we must have

$$\mathrm{OW}=g\frac{M}{L}\mathrm{OV}'.$$

This condition and the condition that VW shall be parallel to OU, already discussed, are together sufficient and necessary for the maintenance of oscillations. In drawing the crank diagram these conditions have to be borne in mind.

From the diagram the formulæ relative to this mode of generating oscillations can be obtained. For this purpose the diagram is repeated in Fig. 4 with the lengths of the lines

marked for convenience. The additional line DN is perpendicular to OW and makes on OW an intercept,

$$
\begin{aligned}
\mathrm{ON} &= \mathrm{OV}' - \mathrm{DQ} \\
&= (L\omega - 1/C\omega)J \\
&= XJ.
\end{aligned}
$$

The parallelism of OU and VW is indicated by marking the angles UOW and OWV each equal to φ.

Project the broken line OUD upon ON and ND. We obtain the two equations

$$
\begin{aligned}
XJ &= RI \cos\varphi + (I/C\omega)\sin\varphi \\
(R+S)J &= (I/C\omega)\cos\varphi - RI \sin\varphi.
\end{aligned}
$$

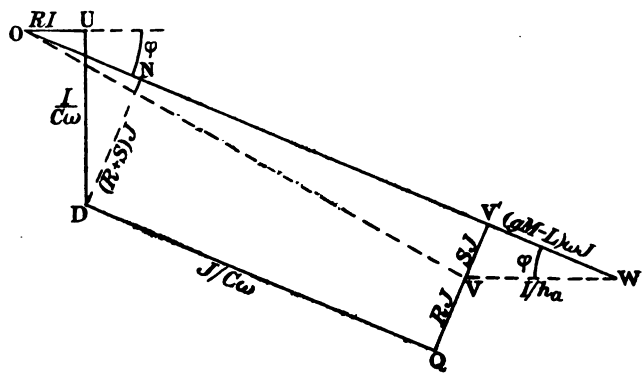


FIG. 4.

But from the triangle VV′W we have

$$
\sin\varphi = \frac{SJ}{I/h_a}
$$

$$
\cos\varphi = \frac{(gM-L)\omega J}{I/h_a}.
$$

Hence
$$
\begin{aligned}
X &= Rh_a\omega(gM-L) + h_a S/C\omega, \\
R+S &= h_a(gM-L)/C - h_a RS.
\end{aligned}
$$

From these we obtain two other equations by solving for $gM-L$ and for ω in turn. The former solution yields

$$
gM = L + C\ RS + \frac{R+S}{h_a}\Big),
$$

which gives the magnitude of the factors concerned in the control—namely, g and M—in terms of the constants of the

oscillator and the conductance of the tube. If the product gM be smaller than required by this equation the oscillations will die away.

Now, solving the above pair of equations for ω, we obtain

$$\frac{1}{\omega^2}=\frac{LC-RC^2(R+S+h_aRS)}{1+h_aS}.$$

This equation leads to an expression for the wave-length of the assemblage in terms of the constants of the tube and the oscillator.

Again, from the equations for sin φ and cos φ, or directly from the triangle VV′W, we have

$$\frac{I}{J}=h_a\sqrt{\{(gM-L)^2\omega^2+S^2\}},$$

which gives the ratio of the amplitude of the sine current through the tube to that of the sine current in the oscillator.

Numerical Example.

Let $L=10^{-2}$ henry, $C=3\times10^{-10}$ farad,
$R=0{\cdot}1$ ohm, $S=100$ ohms,
$h_a=5\times10^{-5}$ mho.

Then
$$gM\doteqdot10^{-2}+3\times10^{-10}(10+100/5\times10^{-5})$$
$$\doteqdot10^{-2}+6\times10^{-4}.$$

If $g=10$, M must be greater than 10^{-3}, for the maintenance of oscillations.

Again,
$$\frac{1}{\omega^2}\doteqdot\frac{3\times10^{-12}-9\times10^{-19}}{1+5\times10^{-3}}\doteqdot3\times10^{-12}.$$

Also,
$$\frac{I}{J}=5\times10^{-5}\sqrt{\{1{\cdot}2\times10^5+10^4\}}$$
$$\doteqdot5\times10^{-5}\times3\times10^2$$
$$=1/70.$$

Let, now, $R=100$ ohms instead of the value above.

Then
$$gM\doteqdot10^{-2}+10^{-3}$$
$$\frac{1}{\omega^2}\doteqdot\frac{3\times10^{-12}-1{\cdot}8\times10^{-15}}{1+1/200}$$
$$\frac{I}{J}\doteqdot5\times10^{-5}\sqrt{\{3{\cdot}3\times10^5+10^4\}}\doteqdot3\times10^{-3}$$
$$=1/30.$$

These results show that the resistance has very slight effects in circuits where the ratio L/C is large, except as regards the ratio of the currents.

Particular Case.

The diagram and the analysis simplify greatly if the inductance coil L is so well designed that its resistance S is zero. This case is approximated to when an antenna is excited direct by an ionic tube, in which case C is the antenna capacity, L its inductance and R the radiation resistance, &c. The diagram appears in Fig. 5 with all its sides marked, and

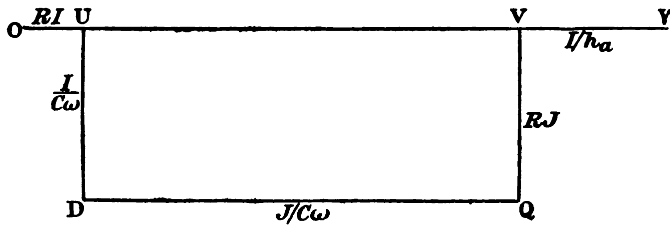


FIG. 5.

needs no explanation. The equations are further easily deduced direct from the diagram or obtained by putting $S=O$ in those above. We obtain

$$gM=L+CR/h_a$$

as the condition assigning the least magnitude to the controlling action, and

$$\frac{1}{\omega^2}=LC-R^2C^2$$

as the frequency equation. The ratio of the currents is

$$\frac{I}{J}=h_a(gM-L)\omega$$
$$=CR\omega.$$

On substituting for ω and squaring, the last equation leads to

$$\frac{J^2}{I^2}=\frac{LC-R^2C^2}{R^2C^2}$$
$$=\frac{L}{R^2C}-1.$$

This points out a limitation not previously mentioned, namely, that

$$L>CR^2.$$

It is noteworthy that in this adjustment of the oscillator it behaves as a mere resistance to the alternating current i; that is to say, it is now non-reactive, and to a steady current it offers no resistance. A formula for the alternating current resistance may be obtained by dividing the length of the line OV in Fig. 5 by the current I, for OV is the amplitude of the P.D. between the terminals of the oscillator and I is the amplitude of the current through it. Calling this quotient Z we may write $OV=ZI$. But

$$OV=L\omega J$$

and therefore $$Z=L\omega J/I,$$

whence $$Z=L/RC$$

by aid of a former equation.

The numerical example given in a preceding paragraph gives the following results when S is taken zero and $R=100$ ohms

$$gM=10^{-2}+6\times10^{-4}\doteqdot10^{-2},$$
$$1/\omega^2=3\times10^{-12}-9\times10^{-16}\doteqdot3\times10^{-12},$$
$$I/J\doteqdot1{\cdot}73\times10^{-2}$$
$$Z=\tfrac{1}{3}\,.\,10^6\doteqdot333{,}000 \text{ ohms}.$$

Control by Condenser Coupling.

A very important circuit with excellent oscillating properties is that of which the connections are given in Fig. 6.

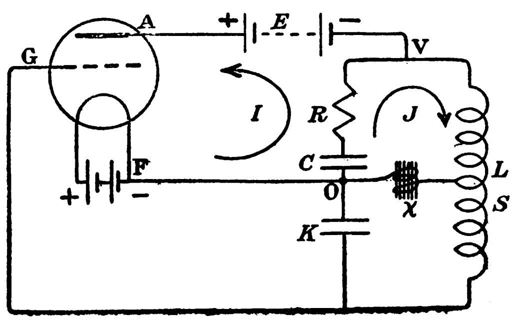


FIG. 6.

We shall assume that when the antenna is made one of the apacitances, that capacitance will be C, and therefore the esistance R may be regarded as antenna resistance. We hall suppose that the inductance L has been designed, as it ught, to be of negligible resistance. A choking coil χ is

connected to the common terminal of the condensers and to a tapping in the inductance so as to provide a path for the steady current from the battery E required to set up the space charge in the tube. The crank diagram is given in Fig. 7, which shows also the notation regarding currents. From it we obtain, as from Fig. 4, the quantitative relations connecting the electrical magnitudes. The angles UOD, DUQ, QVD being equal and the length UV being equal to DQ, give us

$$\frac{J}{I}=\frac{1}{RC\omega}=\frac{I}{XC\omega J}=(Z-R)C\omega.$$

From the first and last we obtain

$$\frac{J^2}{I^2}=\frac{Z-R}{R},$$

and also

$$Z=R+\frac{1}{RC^2\omega^2}$$

The first and second give

$$\frac{1}{R^2C^2\omega}=\frac{1}{XC\omega}.$$

which leads to the frequency equation

$$\frac{1}{\omega^2}=(L-R^2C)C_1.$$

In this, C_1 is written for the capacitance of the condensers C and K in series. From the above we have also

$$Z=R+\frac{(L-R^2C)C_1}{RC^2}=\frac{LC_1}{RC^2}+\frac{RC_1}{K}.$$

In the last equation the former of the two terms is easily the more important. Referring to Fig. 6, we see that the fall of potential from G to F must be due to the current J running in the condenser K, and therefore

$$e_g=j/K\omega.$$

In the crank diagram this fall of potential appears as VP and being in phase with OU we see that the oscillation will be constant if this vector VP is great enough. The control voltage e_g when transferred to the plate circuit is of magnitude g times e_g, and has to make up for the fall of potential

in the resistance of a tube—namely, I/h_a—and for the fall in the oscillator. Thus

$$ge_g=(1/h_a+Z)i=(1/h_aZ+1)v.$$

But
$$e_g=j/K\omega$$
$$=(Z-R)(C/K)i.$$

Therefore
$$g(Z-R)(C/K)=1/h_a+Z.$$

But
$$Z-R=\frac{1}{RC^2\omega^2}.$$

Therefore
$$g/(RCK\omega^2)=1/h_a+R+1/(RC^2\omega^2),$$
$$g/K=RC\omega^2(1/h_a+R)+1/C,$$
$$C/K\doteqdot RC^2\omega^2/(gh_a)+1/g.$$

This shows that the ratio of the two condensers must exceed the value written on the right-hand side of the last equation.

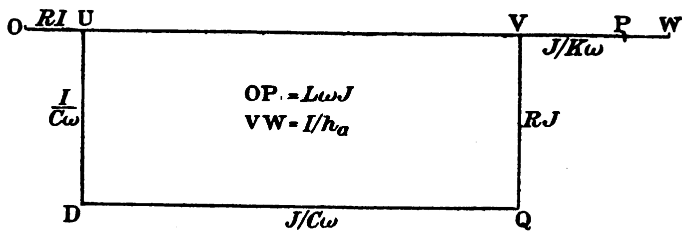


FIG. 7.

The more general case, in which the inductance coil is taken to possess resistance S, is solved by Fig. 8. The grid voltage transferred to the anode circuit must equal the resultant of OV, the fall of potential across the oscillator and VW, the fall of potential across the vacuum, and therefore is represented fully by OW.

The formulæ are obtained from the Figure in a way perfectly analogous to that adopted in Fig. 4. By projection we have

$$XJ=RI\cos\varphi+(I/C\omega)\sin\varphi,$$
$$(R+S)J=(I/C\omega)\cos\varphi-RI\sin\varphi.$$

From the triangle VWV″

$$I\sin\varphi=JSh_a,$$
$$I\cos\varphi=h_a\left(\frac{g+1}{K\omega}-L\omega\right)J.$$

From these we obtain

$$\frac{g+1}{K}=\left\{L+\left(RS+\frac{R+S}{h_a}\right)C\right\}\omega^2$$

as the equation for determining the least magnitude of the control, and

$$\frac{1}{\omega^2}=\frac{L-(R+S+RSh_a)C}{(C+K)/CK+h_aS}$$

for determining the frequency. The ratio of currents is obtained from the first two equations, or directly from the

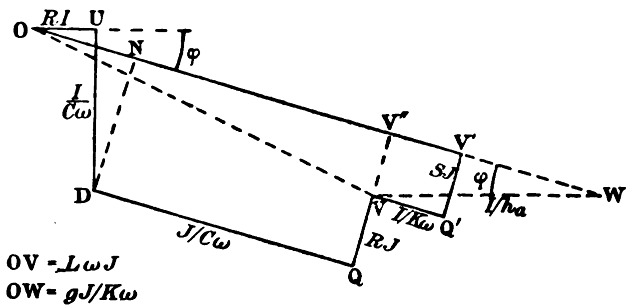


FIG. 8.

NOTE.—The length VQ′ should be marked $J/K\omega$; the length OV′$=L\omega J$.

Figure by expressing OD in terms first of I vectors and then of J vectors; it is

$$\frac{I}{J}=\sqrt{\frac{\left\{L\omega-\left(\frac{1}{C}+\frac{1}{K}\right)\frac{1}{\omega}\right\}^2+(R+S)^2}{(1/C^2\omega^2+R^2)}}.$$

ABSTRACT.

The method of the crank or vector diagrams used commonly in the study of alternating-current circuits is applied in the Paper to the assemblage made up of an oscillator, the thermionic relay maintaining it in oscillation, and the devices linking these two parts. The diagrams then serve as substitutes for the usual treatment of the problem by differential equations and from them may be obtained all the formulæ. They have, besides, the advantage of exhibiting to the eye the phases of the currents and voltages in every part of the circuits. In forming the diagrams the potential drop across the oscillator is calculated by the usual rules of the alternating-current diagram, and added geometrically to the potential drop across the tube. This total is made equal, in magnitude and phase, to the voltage applied at the instant to the grid multiplied by the voltage factor of the relay. In its turn the voltage applied to the relay depends

upon and is obtained from the current running in a portion of the oscillator. The fitting together of these lines gives all the conditions to be satisfied for the maintenance of steady oscillations.

DISCUSSION.

Prof. G. W. O. HOWE agreed that you could not really understand the conditions in a circuit unless you were able to put down the currents and voltages in a vector diagram. He had attempted himself to simplify Vallauri's treatment for students, but without as much success as Dr. Eccles. He had usually looked at these circuits from this point of view: The oscillatory circuit apart from the valve has a certain equivalent resistance. To maintain oscillations the equivalent of a negative resistance must be introduced. There are two ways of varying the current in the bulbs; the P.D. on the terminals or on the grid may be varied. In the latter case the current may be increased even if the total P.D. on the bulb is diminished and if this condition can be arrived at we have the equivalent of a negative resistance to the oscillations. You have to arrange a coupling device so that the variations of current in the plate circuit produces suitable variations of potential of the grid. From the characteristic of the plate circuit you can determine the critical value of the mutual inductance between plate and grid circuits to give the equivalent negative resistance necessary for maintenance of steady oscillations.

Prof C. R. FORTESCUE said the method of approach appears to be a distinct advance, in that the idea of the tube as a generator of an E.M.F., gEg, enables a voltage diagram to be plotted instead of the more usual current diagram. This is an undoubted advance, and simplifies the final adjustment of the diagram to suit the conditions of the tube and circuit. At various times many vector diagrams have been drawn for various oscillatory circuits; and on the whole the results have hardly come up to expectations. There appear to be three reasons for this, viz.: (*a*) In order to draw the diagram at all a very clear insight into the conditions is required. In other words, it is necessary to know the final result before the diagram is completed. In the diagram of Fig. 3 of the Paper it is necessary to know, firstly, that the angle θ is a positive angle, and, secondly, that the effective E.M.F. of the tube is in phase with the voltage applied to the grid. The latter condition is, of course, obvious from the action of the tube, but the former is by no means obvious, and has to be discovered by trial and error. If, for example, the current I is considered to be flowing through the inductance, then θ must be taken as an angle of lag, as will be found if an attempt is made to plot out this diagram. (*b*) The quantities are such that it is impracticable to plot the diagrams to scale. For example, if the numerical values of the first example on page 3 are taken, the ratio of the length of the line OUg of Fig. 3, to the length DQ is of the order of 1 to 500,000. Actually, if Fig. 3 is plotted to scale, it becomes a diagram vertically up and down the board. (*c*) Finally, there is the common experience of the sign difficulty in drawing the diagrams. There are many possibilities of confusion with the ordinary current diagram, but it is possible that with the author's voltage diagram these troubles will be very much reduced. It would appear that the true function of these diagrams, when numerical values are applied or when the diagrams are drawn to scale, is to justify the practical method commonly used of regarding the valve as a power supply. The anode current is in phase with the voltage across the oscillatory circuit. Taking the alternating components only, the product of the anode current and the circuit voltage gives the power supplied to the circuit. The anode current depends upon the anode and grid voltages, *i.e.*,

$$i=h_a e_a+h_0' e_g,$$

as given on page 139 of the Paper. To within a small percentage this power is absorbed by the circuit losses. If $R+S$ is the effective resistance

of the oscillatory circuit, then for the oscillations to be maintained or built up, the power supply from the valve must be equal to or greater than the $J^2(R+S)$ loss. This method of dealing with the problem has many practical advantages, and has been in use for some years. Hazeltine has described, in the "Proceedings" of the American Institute of Radio Engineers, the application of the method to various circuits, notably the circuit in which a condenser is connected across the grid inductance h. It is very well known that the author of this Paper has had experience of many other circuits and applications of three electrode relays, and it is to be hoped that he may be able to give to the Society further Papers on this same subject. In particular, a vector treatment of the De Forest ultraudion circuit would be of great interest, as this circuit is one which has presented very great difficulties when any attempts have been made to calculate the condition for instability.

Mr. J. NICOL asked if there was any lag between the voltage applied to the grid and the effect on the current in the valve.

Dr. D. OWEN said it was assumed that the action of the valve was an amplification of voltage. Actually what happened was a variation in the resistance of the valve. It seemed rather remarkable that this should be regarded as a voltage effect. He did not like Prof. Howe's idea of a negative resistance, as such a conception had no physical significance. A negative value of dV/dC was by no means the same thing as a negative value of V/C, which would be required before we could talk of a negative resistance Did the author find it satisfactory to treat the quantity h_a as a constant ?

Mr. F. E. SMITH thought the Paper of great value. He agreed with Prof. Fortescue that a great amount of interesting work was still to be done in connection with these problems.

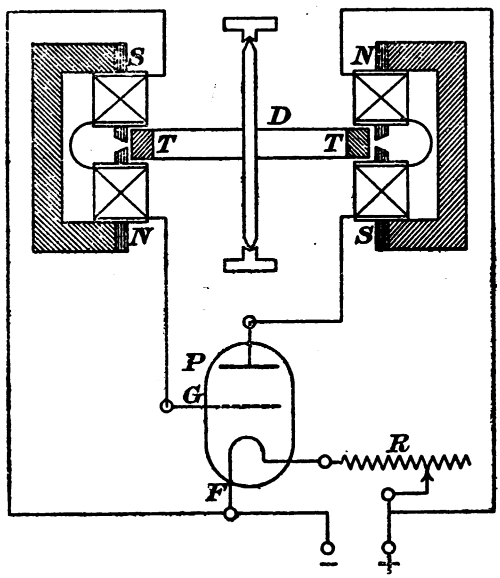
Capt. TURNER referred to a sentence occurring in the section dealing with a particular case: "It is noteworthy that . . . no resistance." This seemed to imply that an infinitesimal change in E would produce a finite change in the steady component of the anode current.

Dr. ECCLES, in reply, said that he did not think vector diagrams were often used quantitatively. They were mainly used to derive a formula, and the scale of the vectors was immaterial. As regards lag, he was not aware of any definite knowledge on this point, but the fact that nowadays waves of 30,000,000 ∞ could be obtained easily showed that the lag must be very small. He thought it was justifiable to assume it zero in slow circuits. The conception of the valve as a voltage amplifier seemed a difficulty; but if we vary the resistance of one part of a circuit containing a fixed E.M.F. it is clear that the P.D. on the remainder of the circuit will also vary; so that the varying resistance can be regarded as a source of varying E.M.F. applied to the remainder of the circuit. The quantity h_a was not strictly constant. As regards Capt. Turner's point, he was referring here to a case in which there was no resistance in the inductance circuit.

XIV. *A Small Direct-current Motor Using Thermionic Tubes Instead of Sliding Contacts.* *By* W. H. Eccles, *D.Sc.*, *and* F. W. Jordan, *B.Sc.*

Received January 15, 1919.

In physical laboratories, especially those in which electric waves and oscillations are studied, circumstances sometimes arise in which a wheel or disc has to be spun rapidly under light load and with absolute freedom from the sparking that occurs in the best ordinary direct-current motor. In such cases a motor employing a rotating magnetic field can be used



if alternating current is available, but often alternating current is not at hand. We therefore describe in this Paper a small perfectly sparkless motor that can be run from a direct current supply, such as that used for lighting. Apart from the applications alluded to, this new motor might be used for maintaining gyrostats in rotation, for driving stroboscopes, and so on.

The motor is an application of the three-electrode ionic relay now so well known. In such relays there is a glowing

filament F functioning as cathode, a plate or cylinder P as anode, and an intervening grid G as control electrode. A constant E.M.F. is applied between filament F and anode P and causes a steady stream of electrons to pass from filament to anode across the vacuum. When a control voltage is applied between filament and grid the anode current increases if the grid is made positive relative to the filament, and it diminishes if the grid is made negative. Either terminal of the filament may be taken as the zero of potential, but it is customary to take the negative terminal. For instance, in the small tubes shown with the motor, the anode current may be about 1·5 milliamperes when the grid is at the same potential as the negative terminal of the filament, and 2·5 milliamperes when the grid is at $+5$ volts and 0·4 milliampere when the grid is at -5 volts. The current flowing into the grid in the first case is 150 microamperes and in the last zero. When an alternating voltage is applied to the control electrode an alternating current appears in the anode or repeat circuit superposed upon the steady current that flows in the quiescent state. This alternating current is capable of doing work, and the power thus made available is much greater than that expended in the control circuit—a fact implied in calling the tube a relay.

In the motor here described a number of iron teeth are carried by the rotating part of the motor past an electromagnet connected into the control circuit of the ionic relay, and these teeth generate in the windings of the electromagnet an alternating E.M.F. that is applied to the grid of the tube. The corresponding alternating current in the repeat circuit is sent through a second electromagnet connected in that circuit and also placed near the rotor. Its position relative to the former electromagnet and to the teeth is so adjusted that the alternating current in it tends to accelerate the movement of the rotor. Put briefly, we may say that the passage of an iron tooth in front of the control magnet applies to the grid an E.M.F. that produces, by means of the relay, a current in the second electromagnet in such a direction as to pull forward the tooth just approaching it. In consequence the spin of the rotor increases until frictional and other losses consume the energy liberated from the battery in the anode circuit.

Obviously a motor constructed on these principles may take many different forms. The one exhibited to-day is sketched in the Figure. The rotor is a horizontal ebonite disc 12 cm. in

diameter mounted on a vertical spindle; the electromagnets were polarised magnets from a pair of 4,000 ohm Brown telephone receivers. The iron teeth are twelve in number and fixed at equal distances on the rim of the ebonite disc.

ABSTRACT.

In this motor the rotating part is an ebonite disc with iron teeth on its periphery, and the stationary part comprises two electromagnets with their poles close to two teeth. One electromagnet is connected to the grid of a thermionic relay, the other is included in the plate circuit. When during rotation a tooth passing the grid magnet induces a voltage in its winding the consequent transient increase of current through the other magnet causes this magnet to exert a pull on the tooth approaching it. We thus have a small motor without commutator or spark which may under no-load be driven up to a speed of 4,000 to 6,000 revolutions per minute from the lighting supply.

CORRECTION.

I find that I was quite in error in the Discussion of my "Note on Microphone Hummers" when I suggested that Lord Rayleigh had sometimes called a mere overtone a harmonic. On the contrary, he has always maintained a clear distinction between the two terms.—ALBERT CAMPBELL.

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XV. *Simplified Inductance Calculations, with Special Reference to Thick Coils.* By PHILIP R. COURSEY, *B.Sc., A.M.I.E.E.*

RECEIVED JANUARY 15, 1919.

1. The predetermination of the inductance of coils by direct arithmetical calculation is a subject to which a great deal of attention has been devoted. The results are seen in the many published Papers giving formulæ for this purpose. Most of these formulæ have been obtained by rigid mathematical deduction, but some are empirical. Many are very complicated, and unsuited for easy computation.

Several writers have attempted the simplification of some of these formulæ to render them more suitable for everyday use. Often this has resulted in a limitation of their range of applicability to certain selected cases, and in some instances confusion has arisen through imperfect statement of the limits between which the particular formula may be employed.

2. *Notation.*

Let—

L=Inductance of coil in centimetres.
D=Mean diameter of coil (cm.)$=2a$.
a=Mean radius of coil.
l=Axial length (cm.).
d=Radial depth of windings (cm.).
N=Total number of actual turns of wire.
$n=N/l$=Number of (actual) turns of wire per centimetre of length.
n'=Number of (imaginary) turns of square wire that can be fitted into coil section$=l/d$.
k=Correction factor for single layer windings.
δk=Correction for coil thickness.
k'=Correction factor for thick coils.
$\Delta_1 L$=Rosa's Correction for coil thickness.
$m, A, B,$ &c.=Other factors used in various formulæ.

3. *Single Layer Coils.*

The calculation of the inductance of single-layer coils has, perhaps, received most attention in the direction of simplification on account of the practical requirements in the realm of wireless and other high-frequency apparatus. Several

abacs, charts and curves have been published with this end in view.

When dealing with single layer coils one of the most useful, and, at the same time, most accurate and universal of the available formulæ is that of Nagaoka.* The main part of this formula, viz. :—

$$L=\pi^2D^2n^2lk \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

is very easily dealt with, but the expressions for calculating the factor k for the various cases of long or short coils are very cumbersome and tedious to use. Extensive tables of this factor have, however, been published in the "Bulletin" of the Bureau of Standards, and elsewhere, giving the values of k in terms of the ratio D/l=diameter÷length of the coil.

The use of this formula in practice may be simplified by plotting curves of the values of k and reading the value required from these. This method has other special advantages when the design of a coil to have any predetermined value of inductance is under consideration, as has been previously pointed out by the author.† The general form of this function k plotted against the ratio l/D is given in Fig. 1.

The results given by this formula are, strictly speaking, liable to a correction for the insulation or spacing of the turns of wire, but in practice this correction is a small one (usually less than 1 per cent.), and may be neglected—at least to the approximation required for most practical work—for which curves are applicable.

This formula may be expressed in a number of different ways if so desired. Such modifications have given rise to a number of abacs and charts to simplify its use. In general, however, they amount to the same result as that given by the curves above, while their range is usually much more limited.

As an example we may mention Eccles's abac given in his "Handbook of Wireless Telegraphy." This is based on Russell's formula, and is written in the form

$$L=ma^3n^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Values of m are obtained from the abac in terms of the ratio l/D (Fig. 2).

* Nagoaka, "Journal" College of Science, Tokyo, XXVII., p. 18 (1909); also "Bulletin" of Bureau of Standards, Washington, VIII., p. 119 (1912).

† P. R. Coursey, "Electrician," LXXV., p. 841 (September, 1915).

If we equate this expression to the one used for the curves of Fig. 1, we have

$$m(D/2)^3n^2=\pi^2D^2n^2lk,$$

or,

$$m=8\pi^2\frac{lk}{D}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

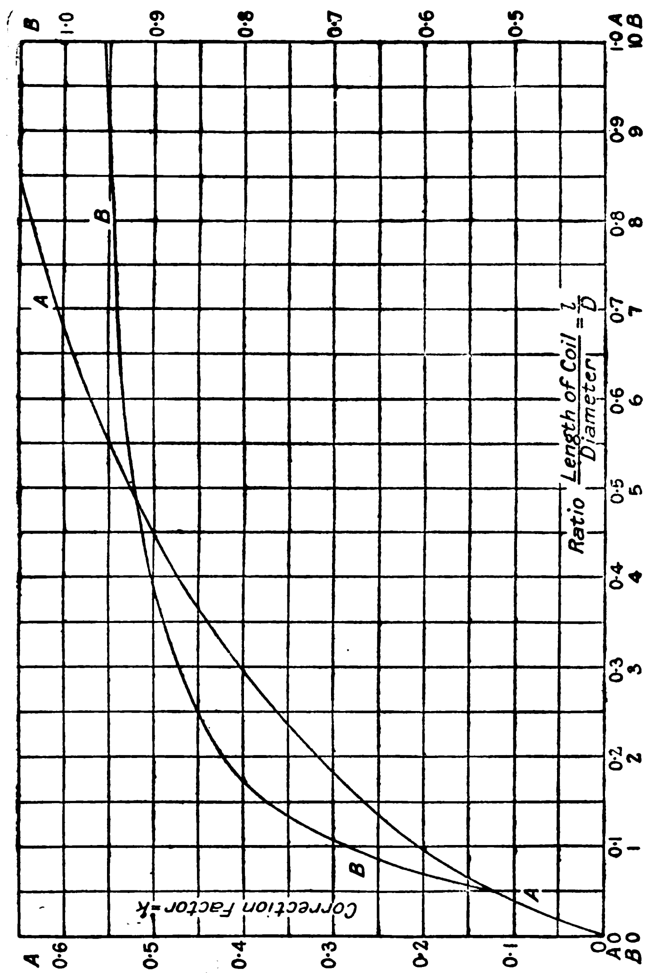


Fig. 1.—Curves of Correction Factor for Single layer Coils.

We may therefore compare the results obtained by the two methods by working out values of $8\pi^2[lk/D]$.

Curves of lk/D were given by the author in the "Electrician"* for the purposes of the design of coils. Using these curves, we obtain the following table :—

l/D.	lk/D, from curve.	$8\pi^2 lk/D$.	m from abac.	Difference, per cent.
0·2	0·063	4·96	5·00	+0·8
0·65	0·384	30·3	30·2	−0·33
1·5	1·15	90·8	91·0	+0·22
2·4	2·04	161·0	160·0	−0·62
4·2	3·80	300·0	300·0	0·0
5·2	4·78	384·0	379·0	−1·3

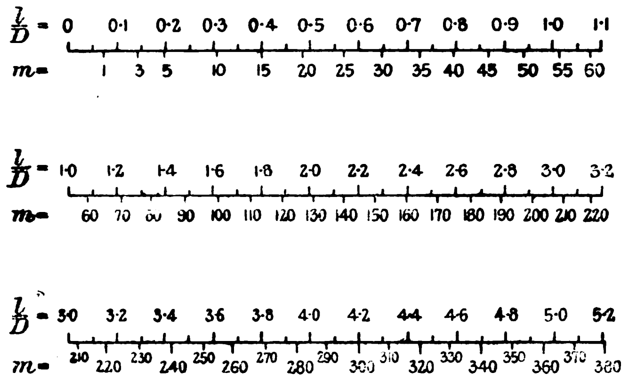


Fig. 2.—Abac for Single-layer Coils (Dr. Eccles).

The agreement is evidently very close. The useful range of the abac is, however, much more limited than that of the curves. It is only useful for comparatively short coils.

A very similar chart due to S. Lowey, published in the "Wireless World,"† amounts to practically the same abac arranged in a circular form, but with a slightly greater range, and expressed in terms of the coil diameter instead of its radius.

A disadvantage of transforming Nagaoka's formula in this manner, viz.: splitting it up into factors of D^3, and lk/D, is that the factor m, which is proportional to lk/D, covers a much greater range of values—varying between 0 and ∞ —whereas the factor k is asymptotic to unity, and its value for all cases lies between 0 and 1. A curve or abac for k does not, therefore, require to be so extended.

* P. R. Coursey, "Electrician," *loc. cit.*
† S. Lowey, "Wireless World," III., p. 664 (January, 1916).

4. *Thick Coils.*

When we come to consider the case of coils having a radial thickness that is not negligible compared with either the diameter or the length, these simplified formulæ become very inaccurate if used as they stand. Moreover the usual accurate expressions for these cases are not easy to work with in the forms usually given. These coils—"thick coils," as we may term them for distinctive purposes—have not perhaps quite such a wide sphere of use as the single layer coils, but nevertheless they are of importance in some branches of engineering, and are finding more extended use for high-frequency work than hitherto.

It is understood, of course, that these calculations always refer to "air-core" coils, or to be more general, to all coils having a core of permeability unity, as the inductance of iron-cored coils is too uncertain and variable a quantity to require predetermination in most cases.

The principle object of this Paper is to indicate how the simplified form of Nagaoka's formula, using the "k" curves, may be adapted to a general method of calculation applicable to all types of coils, whether thin or thick, short or long, of one turn or many.

In the single layer case considered above, the magnetic flux passing through the centre of the coil is linked with practically all the turns—the factor k correcting for the spreading of the flux at the ends of the coil. Evidently when more layers are added to the coil, the flux due to the inner layers does not link directly with the outer layers of wire, so that the effective inductance is less than it would be if the same number of turns were all concentrated upon a single outer layer. It should, therefore, be possible to allow for this reduction in inductance by introducing an appropriate reduction in the factor k. For this purpose a series of values of a factor δk have been calculated. This factor is to be subtracted from the proper value of k obtained from the curves, and the new value k' used with the standard formula in the usual manner.

Hence we have

$$L=\pi^2D^2n^2l(k-\delta k)$$
$$=\pi^2D^2n^2lk', \qquad \qquad (4)$$

where $k'=(k-\delta k)$.

A series of values of this reduction factor δk is shown in Fig. 3.

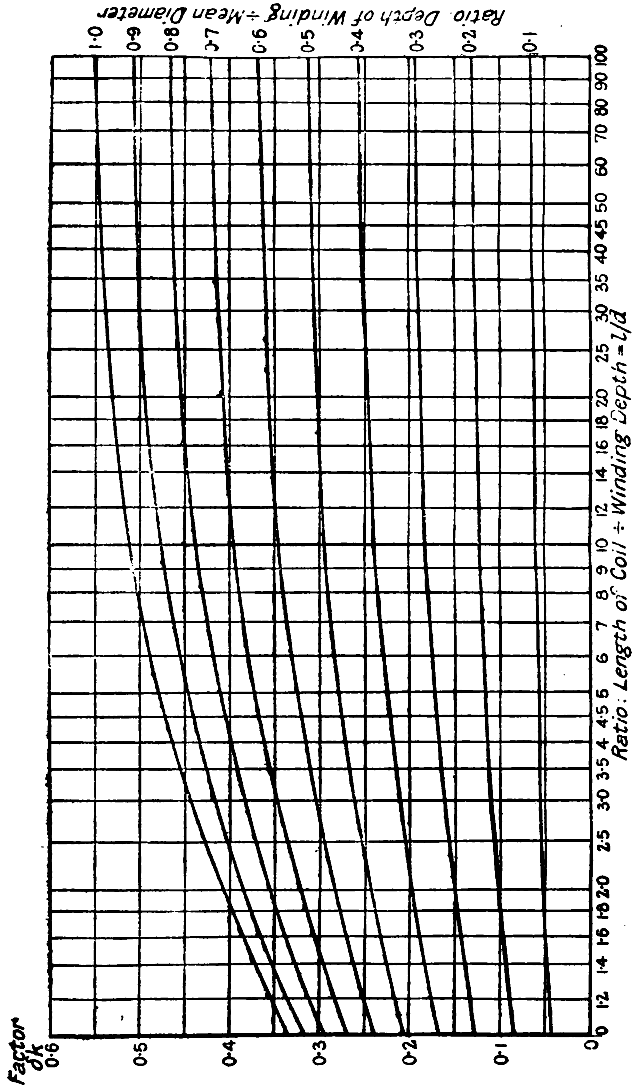


Fig. 3 —Curves of Factor δk for Thick Coils.

The method by which they were obtained may be indicated as follows :

Using Rosa's formula for the inductance of a thick coil—one of the most accurate for this class of coils—we have at once an expression in the form required.

This is

$$L = L_s - \Delta_1 L + \Delta_2 L, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where L_s is the inductance calculated by any suitable formula for " current-sheets " or infinitely thin single-layer coils ; and $\Delta_1 L$ is a correction for the coil thickness given by

$$\Delta_1 L = 4\pi a n' [A_s + B_s]. \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

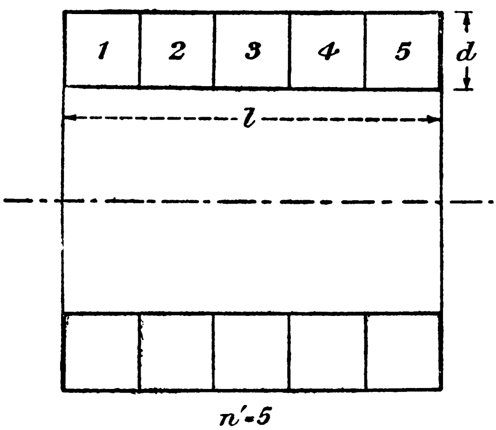


FIG. 4.

A_s and B_s are factors tabluated by Rosa in the " Bulletin,"* $\Delta_2 L$ is a correction for the insulation of the wires, and, in most of the cases here dealt with, is quite negligible. The term n' in the expression (6) is the number of square conductors that can be fitted into the coil section, Fig. 4. Evidently $n' = l/d$.

The " current-sheet " inductance must be worked out for this number of turns n' to enable the correction $\Delta_1 L$ to be properly applied. The final result must be corrected to allow

* E. B. Rosa, " Bulletin " of Bureau of Standards, IV., p. 369 (1907); also VIII., p. 138 (1912).

for the actual number of turns N on the coil. Thus, using Nagaoka's formula for L_s, we have

$$
\begin{aligned}
L &= L_s - \Delta_1 L \\
&= \frac{N^2}{n'^2}\left\{\frac{\pi^2 D^2 n'^2}{l} k - 2\pi D n'(A_s + B_s)\right\} \\
&= \frac{\pi^2 D^2 N^2}{l}\left[k - \frac{2}{\pi} \cdot \frac{l}{D} \cdot \frac{1}{n'}(A_s + B_s)\right] \\
&= \frac{\pi^2 D^2 N^2}{l}(k - \delta k),
\end{aligned}
$$

where
$$
\begin{aligned}
\delta k &= \frac{2}{\pi} \cdot \frac{l}{D} \cdot \frac{1}{n'}(A_s + B_s) \\
&= \frac{2}{\pi} \cdot \frac{d}{D} \cdot (A_s + B_s). \quad \quad (7)
\end{aligned}
$$

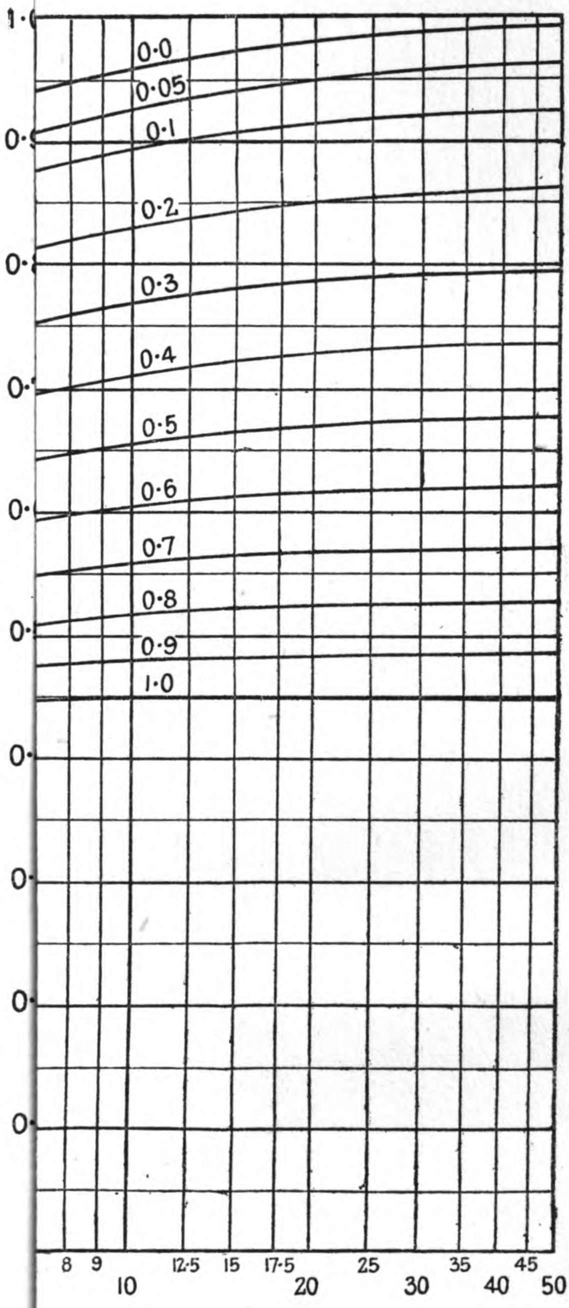
The values of δk have been calculated from this expression by using the values for A_s and B_s tabulated by Rosa. A considerable number of extra values have also been calculated from the formulæ given in his original Paper,* as the tables were found insufficient for this purpose.

Evidently not only is it possible to plot curves of δk, as has been indicated in Fig. 3, but a set of curves of the factor $k' = k - \delta k$ may also be plotted on one sheet to enable the appropriate factor for *any* coil to be read off at a glance. (*See* Fig. 5.)

These curves are plotted against the ratio l/D (=length ÷diameter), so that the k curve for a single layer coil can also be included. A wide range of values is obtained by the use of the logarithmic scale. In using the curves it should be noted that when the winding depth d is greater than the axial length l, the values of l and d should be interchanged in working out the inductance both in the formula and in finding the factor k'. The inductances of the short thick coil and the long thin one of Fig. 6 are practically the same, provided that the mean diameters are identical.

The results obtained from these curves have been compared with measurements on several coils, and have been found to be generally correct within a few per cent.

* E. B. Rosa, *loc. cit.*



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17·5
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45
50

[To face p. 162

These k' curves have an advantage in being asymptotic as compared with others, in which the formula has been broken up into factors of n^2, D^3, and m, as for the single layer case (Eccles's Abac, &c.), equation (2), or into factors of N^2, D, and $(A-B)$, as in L. A. Doggett's chart.* This last is shown in Fig. 7.

The values given by this chart compare very well with the k' curves in spite of the rather small scale of the chart as pub-

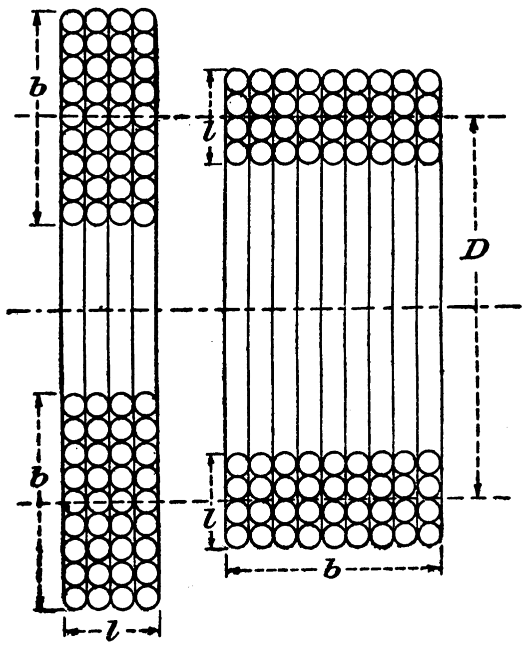

FIG. 6.—EQUAL INDUCTANCE COILS.

lished. Taking the example quoted on the chart (Fig. 7) the inductance using the k' curves is 2,190 cm., while that given by Doggett is 2,130 cm. This is rather an unfavourable case, and most results are nearer than this one. The k' curves in nearly all cases give a much more accurately readable value of the correcting factor.

The method of calculation adopted for the factor δk does not, in its present form, admit of its calculation for values of l/D

* L. A. Doggett, "Electrical World," January 31, 1914; "Electrica Review" (Lond.), LXXIV., p. 489 (1914).

$<d/D$, and it will be noted that in consequence the k' curves as given do not extend beyond this value. This point corresponds to the square section coil, length=depth. Shorter coils may be calculated to a first approximation by interchanging

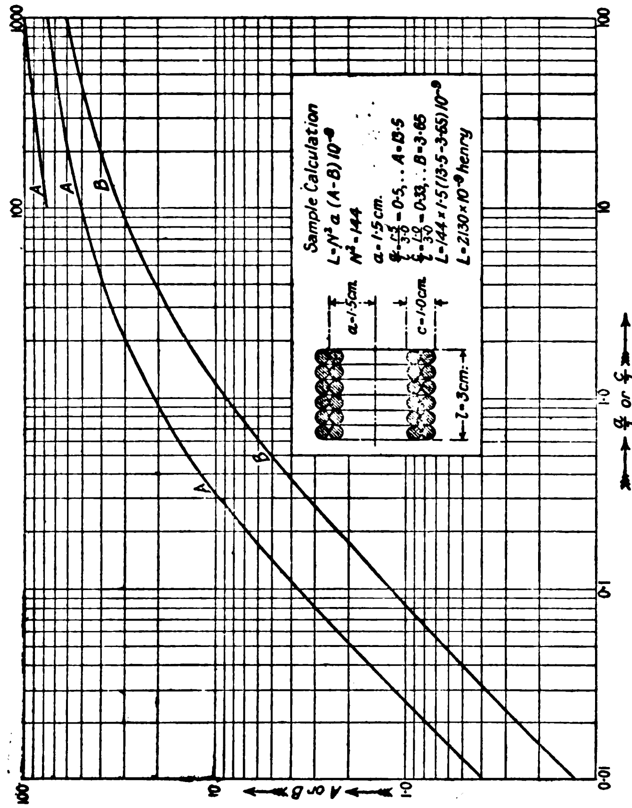

Fig. 7.—Chart for Thick Coils (L. Doggett).

l and d as indicated above. It is hoped in a later communication to deal with the extension of these curves to the smaller values of l by an alternative method of calculation.

6. *Single-turn Rings.*

Another use for these curves should also be noted—viz., their application to coils of very small section, down to the case of single turns of wire.

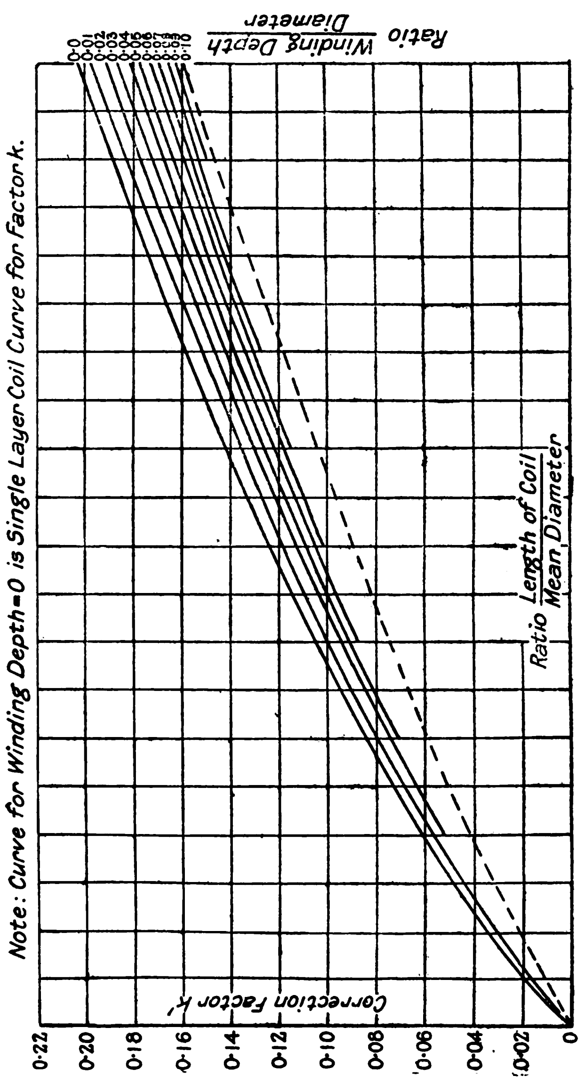

FIG. 8.—CURVES OF k' FOR COILS OF VERY SMALL SECTION (=Lower part of Fig. 5 to enlarged scale).

Taking the latter as an extreme case, it may be regarded approximately as a "square-section" coil of length=depth =diameter of wire. As an example :—

A single turn of wire of mean diameter $D=10$ cm. and wire diameter=0·5 cm. :—

$$l=d=0{\cdot}5. \quad l/D=0{\cdot}05=d/D$$

$$\therefore\ k'=0{\cdot}1015.$$

$$\therefore\ L=\pi^2\times\frac{10^2\times1^2}{0{\cdot}5}\times0{\cdot}1015.$$

$$=200 \text{ cm.}$$

The actual inductance of this ring=192·2 cm.

To render this method of calculation more easily applicable to these cases, a series of values of k' for very small values of l/D and d/D have been calculated. They are set out in Fig. 8.

To determine the initial values of k (*i.e.*, $d/D=0$) required for these low-range curves, it should be noted that Rayleigh and Niven's formula for the inductance of short coils may be thrown into the same form as Nagaoka's. A comparatively easy method of calculation for the small values of k is thus obtained. Nagaoka's expression for this case is very cumbersome, and the values are outside his tables.

Rayleigh and Niven's formula is

$$L=4\pi aN^2\left[\log_e(8a/l)-\tfrac{1}{2}+\frac{l^2}{32a^2}\left(\log_e\frac{8a}{l}+\tfrac{1}{4}\right)\right]. \quad . \quad (8)$$

Equating this to $\pi^2D^2n^2lk$, we have

$$k=\frac{2}{\pi}\cdot\frac{l}{D}\cdot\left[\log_e\left(4\frac{D}{l}\right)-\tfrac{1}{2}+\tfrac{1}{8}\left(\frac{l}{D}\right)^2\left(\log_e\left(4\frac{D}{l}\right)+\tfrac{1}{4}\right)\right]$$

$$=\frac{2}{\pi}\cdot\frac{l}{D}\left[\log_e 2{\cdot}4261\,\frac{D}{l}+\tfrac{1}{8}\left(\frac{l}{D}\right)^2\log_e 5{\cdot}1210\,\frac{D}{l}\right]. \quad . \quad (9)$$

These low-range curves of k' do not extend to values of l/D less than d/D, for the reasons already given.

In conclusion, it is hoped that this indication of a uniform and easy mode of calculation of the inductance of all ordinary shapes of coils may be of some utility in practical work, by limiting the number of formulæ to be memorised or to become familiar with, and thus leading to less confusion and error.

ABSTRACT.

The method of calculation advocated in the Paper is based on an extension of Nagaoka's formula for single layer coils, to include as well all ordinary forms of thick coils. Rosa's formula for thick coils is put into the same from as Nagaoka's, and its use enables a series of correction factors to be calculated for various coil thicknesses. By the aid of a single sheet of curves giving values of these correction factors the inductance of any form of coil likely to be met with in practice may be readily calculated, using only one simple standard formula for all cases. Reasonable accuracy is obtained even in the limiting example of a single turn of wire. The results arrived at agree well with other published charts, which are usually of more limited application, while the use of a single formula for all cases lessens the liability to error.

It is also shown that Rayleigh and Niven's formula enables the calculation of the correction factors for very short coils to be carried out without having recourse to Nagaoka's more complicated expressions.

DISCUSSION.

Dr. ECCLES said that as the author had mentioned an abac given in the speaker's book on "Wireless Telegraphy," it was well to remark, first, that this abac was deliberately made of its present range so as to provide an open scale for coils of the proportions used in practice. For the rare cases of coils outside the range of the abac very simple formulæ are available. Dr. Russell's formulæ were used in making the abac, because they seemed more convenient for the purpose than the results of Nagaoka, to which they were, of course, equivalent. By constructing new curves for dealing with coils of several layers the author has rendered a great service, and has opened easy paths through a forest of laborious theoretical calculation. Now that this has been done it will be much easier to compare the measured values obtained on actual coils with the calculated values, and so we shall be aided to accumulate experience of the effects of high frequency eddy currents on the inductance of coils.

Prof. HOWE agreed with Prof. Eccles' remark about the general utility of Mr. Coursey's investigation. The chief difference between the author's curves and those of the Bureau of Standards was that in the latter the ratio D/l and k, which was a function of D/l, were combined into a single function.

Mr. NICOL referred to formulæ published in "The Electrician" by Mr. Coursey a few years ago, and considered that the step now taken, which resulted in restricting the values of k to between 0 and 1, combined with the general inclination of the curves to about 45 deg. with the axis, gave much greater convenience in reading.

XVI. *Some Characteristics of the Spark Discharge and its Effect in Igniting Explosive Mixtures.* By CLIFFORD C. PATERSON, *M.I.C.E., M.I.E.E.*, and NORMAN CAMPBELL, *Sc.D.*

RECEIVED FEBRUARY 19, 1919.

NOTE.—The work described in this Paper was carried out at the National Physical Laboratory at the instigation of the Advisory Committee for Aeronautics. The results have been communicated in a series of confidential reports to the Internal Combustion Engine Sub-Committee of that Committee, who have now given permission for the publication of any parts of it which appear of general scientific interest.

PART I.—THE NATURE OF THE SPARK.

[FOR SUMMARY SEE P. 196.]

INTRODUCTION.

1. *Objects of the Research.*

The work about to be described was undertaken with the ultimate object of determining whether any considerable improvement could be made in the electric ignition of explosive engines. The electric discharge used for this purpose is always produced either by a magneto or an induction coil, instruments which are essentially similar in principle and give discharges which probably do not differ very greatly in their fundamental characteristics. It was thought that a more complete study of the characteristics of the discharge which are necessary and sufficient to secure satisfactory ignition might possibly indicate that advantages could be secured by adopting some totally different system of producing the discharge. It appeared unlikely that the electrical advantages which any other system might be found to possess would be great enough to counterbalance the mechanical advantages of the present system, which has been so highly developed; but in view of the possibility of such a discovery, it was decided to make the investigation as thorough and as fundamental as possible and not to confine the attention to discharges produced by methods similar to those used in practice.

2. *Previous Work on Electric Ignition.*

The study of the electric ignition of explosive mixtures has already a voluminous literature. Much of the work has been directed mainly to the chemical side of the problem, to the determination of the " limits of inflammability " in the variation of the proportion of fuel and air, and generally to the investigation of what mixtures will be ignited by a given sparking arrangement. The problem which is attacked in this research is rather what sparking arrangement will ignite a given mixture ; on this problem light is thrown by a comparatively small proportion of the previous work. The following Papers (which will hereafter be denoted by the numbers attached to them) appear to contain most of the work that has been published in recent years which has any bearing on the present investigation.

(1) H. F. Coward, C. Cooper and C. H. Warburton. Chem. Soc. " Journ.," CI., pp. 2278-2287, December, 1912.

(2) H. F. Coward, C. Cooper and J. Jacobs. Chem. Soc. " Journ.," CV., pp. 1069-1093, April, 1914.

(3) W. M. Thornton. Roy. Soc. " Proc." A., XC., p. 272, 1914.

(4) W. M. Thornton. Roy. Soc. " Proc." A., XCI., pp. 17-22, November, 1914.

(5) W. M. Thornton. " Phil. Mag.," XXVIII., pp. 734-738, November, 1914.

(6) W. M. Thornton. Roy. Soc. " Proc." A., XCII., pp. 9-22, October, 1915.

(7) S. G. Gastry. Chem. Soc. " Journ.," CIX., 423-529, May, 1916.

(8) W. M. Thornton. Roy. Soc. " Proc." A., XCII., pp. 381-401, May, 1916.

(9) W. M. Thornton. " Phil. Mag.," XXXIII., pp. 190-196, February, 1917.

(10) R. V. Wheeler. Chem. Soc. " Journ.," CXI., pp. 130-138, February, 1917.

(11) R. V. Wheeler. Chem. Soc. " Journ.," CXI., pp. 411-413, May, 1917.

The explosive mixtures used in these experiemnts consisted almost always of hydrocarbons (usually pure), including hydrogen, with air or oxygen. Three different methods of producing the spark were employed. In (3), (5), (6) a " break spark " was used—that is to say, a constant current, direct or alternating,

flowing in a metallic conductor was interrupted by breaking the circuit at a point within the explosive mixture. In (1), (2), (7), (8), (9), (10), (11) the spark was produced across a constant gap by breaking the current in the primary of an induction coil, of which the secondary terminals were connected to the gap; the spark produced by this method is usually termed by the authors who used it "the impulsive electric discharge," or sometimes "the induction spark." In (4) and (5) a "condenser discharge" was used, produced by approaching two terminals, one connected to each coating of a condenser charged to a known potential.

Whichever method was adopted, it was always found that a spark could be passed through the mixture without causing ignition, provided that the spark was of sufficiently low "intensity." The experiments consisted in determining the least "intensity" which would ignite various mixtures. As a measure of this "intensity," necessary to express quantitative results, the value of the current broken was used in the case of the "break spark," the value of the current interrupted in the primary when the "induction spark" was used, and the values of the capacity of the condenser and the original potential to which it was charged when the "condenser spark" was employed.

The results obtained with the "break spark" and the "induction spark" have considerable direct practical importance—the former in determining the conditions in which explosions are likely to be initiated in mines, the latter in application to the coil ignition of explosive engines. But the latter, if not the former, are not easy to generalise; and it is still more difficult to co-ordinate either with the other or with the results obtained with the "condenser spark." For the critical "intensity" which will just explode a mixture certainly varies with the precise experimental arrangements employed. When the induction coil is used, the critical intensity will vary with the construction of the coil; when the "break spark" is used it will probably vary with the manner of break. Further, none but a purely empirical relation can be stated between the critical intensities for the same mixture measured by the three different methods of starting the discharge. Enough is not known of the mechanism of the discharge to formulate any general proposition about the relation between the discharge produced by breaking a certain current in the primary of an induction coil (even if the construction of the coil is fully

known) and that produced by approaching two terminals connected across a condenser of given capacity charged to a given voltage.

In other words, the result of all these experiments are stated in terms of the properties of the instrument used for producing the spark; they are not stated in terms of the properties of the spark itself, and it is doubtless the properties of the spark itself which determine its igniting power. That power must depend in some way on such characteristics of the spark as the average or maximum current through it, the time that it lasts, the potential between the terminals, the variation of that potential with the time, and so on. It is only if the igniting properties of the spark can be defined in terms of such characteristics that a result will be reached which is really general and permits a prediction of the igniting power of any spark produced by any experimental arrangement or a consideration of the best arrangement to produce a spark of given igniting power. To obtain such a result is the ultimate object of the research, and previous work does not give much help towards its attainment.

There are, however, a few general conclusions which may be deduced:—

1. With the induction and condenser sparks the critical intensity for a given mixture decreases if the spark potential is increased. In the break spark there is, of course, no spark potential. The spark potential in a given mixture and with given electrodes can only be changed by changing the sparking distance; few direct observations on the change of critical intensity with the spark gap were made, but all of them showed a marked decrease of critical intensity with increase of spark length. The spark potential can also be changed by altering the form of the electrodes, but the experiments are not sufficient to decide how the critical intensity varies with such changes. It can also be changed by changing the pressure of the mixture; and in this case the critical intensity again was found to decrease as the spark potential was increased. But since, for our purpose, a mixture of the same chemical composition at a different pressure must be regarded as a different mixture, such observations cannot be taken to show certainly that the critical intensity decreases with increase of spark potential in the same mixture.

2. The materials of the electrodes have little or no effect on the igniting power of the discharge. Thornton found some

effect due to the material of the electrodes, but his results have not been confirmed by others, and are capable of an explanation (to be discussed later) different from that which he puts upon them. In the " break " spark the material of the electrodes may have some effect, but since such a spark is really an arc, in which the discharge is conditioned by the temperature of the electrodes, this conclusion has no bearing on the true spark.

3. Much the most important conclusion to be drawn concerns the energy associated with the critical intensity. Among engineers the idea appears to have been widely prevalent that the energy dissipated in the spark was the determining factor in its igniting power. When this research was undertaken one of the most common methods of testing the efficiency of a magneto was to determine the energy dissipated in the spark which it gave between given electrodes in a given atmosphere ; it appears to have been believed that the magneto which gave the greatest " joules per spark " would prove the most efficient igniting agent. The idea was not unplausible in the absence of any experimental evidence, and would naturally follow from any theory which regarded ignition as a purely thermal process. But the evidence contained in the papers cited is quite sufficient to show that the idea is erroneous. Most of the writers gave estimates (not always very reliable, but sufficient for our purpose) of the energy dissipated in the spark of critical intensity for a given mixture, and a comparison of their figures shows how widely this energy varies with the method by which the spark is produced. The energy of the critical break spark is often 100 times as great as that of the critical condenser spark in the same mixture. Moreover, Thornton's work enables the energy of different critical condenser sparks to be compared ; he found that the energy, as well as the " intensity," of the critical spark decreased very notably as the sparking potential was increased.

It is curious that so many writers continue to express their results in terms of the energy of the spark when their own results show very clearly that the energy is not the determining factor in igniting power. The practice is probably due in part to the hope that, by means of the conception of energy, they may be able to express a relation between the igniting power of sparks produced by different means ; if it were true that the igniting power of a spark is determined by the energy

in it, the problem of expressing igniting power in terms of the property of the spark, and not merely of the means of producing it, would be solved. But it is certainly not true, and a much deeper investigation is needed to discover what are the characteristics of the spark which determine its powers of ignition.

3. *Preliminary Experiments.*

It was thought at the outset that the two characteristics of the discharge which were most likely to determine its igniting power were the current density and the time that the discharge lasted. Accordingly an attempt was made to produce a form of discharge in which these two factors would be separately controllable. The known properties of the discharge at low pressure suggested that such control might be obtained, for at a pressure of 2 mm. or 3 mm. it is easy to find conditions such that the current carried by the discharge is a function of the form of the electrodes and the P.D. maintained between them, and remains constant as long as the P.D. is maintained. No evidence was known that similar conditions could not be obtained at much greater pressures, and it was expected that the chief difference which would be found between the discharge at atmospheric pressure and that at low pressures would be a reduction of the length scale of the discharge consequent on the reduction of the mean free path of the ions.

Accordingly the apparatus shown in Fig. 1 was set up, or, rather, part of it, for some of the portions which are shown and will be described later, were added subsequently.

S is the spark gap immersed in the explosive mixture; the form of the terminals and the distance between them were varied in different experiments. In the preliminary experiments the explosive mixture consisted of hydrogen and air, and was always at atmospheric pressure.

Current was supplied to the spark gap from a constant source of potential. At first high potential dynamos were used for this purpose, but later a more convenient arrangement proved possible. It was soon found that the current which it was necessary to supply and the times which the current had to last were so small that the total quantity of electricity which passed in any one experiment was at most only a few micro-coulombs. Accordingly, the supply was taken from the condenser C_1 (a battery of Leyden jars with a capacity of about 0·1 mfd.), which could be charged up to any desired potential by a high-tension transformer, fed with alter-

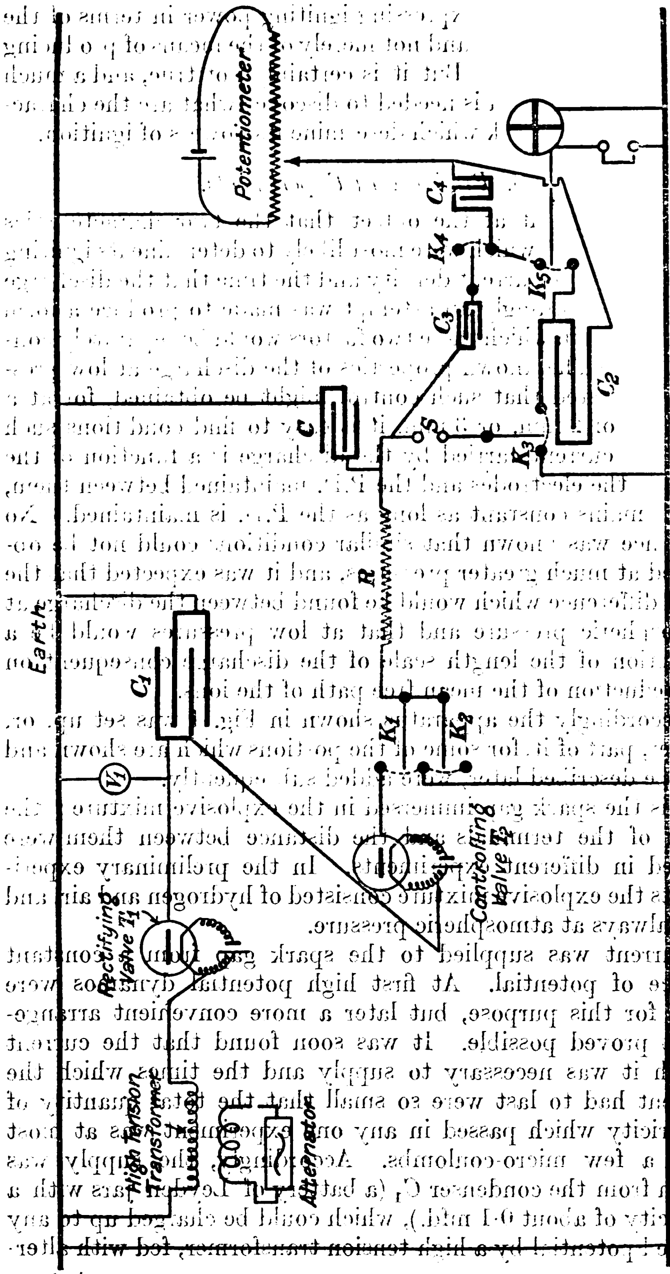

FIG. 1.—CONNECTIONS OF APPARATUS FOR TUNING SPARK

nating current, and connected to the condenser through a thermionic valve T_1. The potential to which the condenser was charged was read by the electrostatic voltmeter V. So long as the total quantity of electricity which passes in a single experiment is a small fraction of that contained in C_1 when it is fully charged, C_1 can be regarded as a source of constant potential and used to replace the high tension dynamos.

Between the source of potential and the spark gap were inserted devices which, it was hoped, would control the current passing through the spark and the time that it lasted. In order to control the current a second thermionic valve T_2 was used. Since the valve was very highly evacuated, the current through it was approximately saturated and determined only by the temperature of the filament, and not by the P.D. across it. As a matter of fact, the saturation was not perfect; but the current only increased some 30 per cent. when the P.D. was increased from 500 to 10,000 volts, and it might be assumed with sufficient accuracy that the current passing through the valve was independent of such changes in the P.D. across it as were likely to occur when the discharge was started or stopped, so long as that discharge was of the nature which was expected.

In order to control the time that the discharge lasted the switches K_1 and K_2 were inserted. K_1 connected the valve to the spark gap, and so started the discharge; K_2 connected metallically the terminals of the spark gap, and thus stopped the discharge. These two switches, immersed in oil, were operated through electromagnets by a Helmholtz pendulum, which broke in succession two contacts, each of which short-circuited one of the electromagnets. One of these contacts could be moved relatively to the other by a micrometer screw, so that the time interval between the breaking of the contacts could be varied at will. The speed of the pendulum when it struck the contacts was 300 cm./seconds; the distance apart of the contacts could be changed by steps of 0·01 mm.; so that the time interval between the breaking of the contacts could be changed by steps of 3 micro-seconds. Owing to the delay in the action of the electromagnets, the interval between the operation of K_1 and K_2 was not equal to that between the breaking of the contacts by the pendulum; but, if everything else was unaltered, a change in the latter interval produced an equal change in the former.

The second terminal of the spark gap was connected to apparatus for measuring the quantity of electricity which passed through the discharge. One side of the condenser C_2 was connected to the spark gap through the switch K_3, and to the Dolazalek electrometer O; the other side was connected to earth through the potentiometer. If K_3 was closed, the electricity passing in the discharge was received in C_2, producing a deflection of the electrometer which could be compensated by means of the potentiometer. If v is the potential which must be imposed by the potentiometer to bring the needle back to zero, the quantity of electricity which has passed is C_2v. K_3 could be operated by the pendulum in place of K_2, but in the preliminary experiments it was always closed. The resistance shown at R was not present in these experiments.

4. *Preliminary Results.*

In taking observations the external potential, shown at V, was first fixed at some value considerably greater than the spark potential of the gap, and the filament temperature of T_2 fixed so that the current flowing through the valve had some value lying between 0·00001 and 0·1 ampere. Then, starting with the pendulum contacts in such a position that K_2 was known to operate before K_1, the operation of K_1 was advanced until a spark first passed across the gap when the contacts were operated by the pendulum. The corresponding reading of the micrometer screw should then correspond to the simultaneous operation of K_1 and K_2, and the interval corresponding to any other reading could be deduced.

It should be noted, in passing, that the eye could not be trusted to determine whether or no a spark had passed; the only safe criterion was the occurrence of a deflection in the electrometer; such deflection would often occur when no spark had been seen. The failure to see the spark was doubtless due to a wandering of the eye during the very short period during which it lasted. But when they are seen, the visibility of small sparks of very short duration presents some features of interest. As the time during which the spark lasted was increased, the apparent luminosity of the spark increased also, until the duration was so long that the spark appeared to the eye to last a finite time. After this stage was reached, an increase in duration did not produce any increase in apparent luminosity.

The object of the observations was to determine how long the spark, passing a known current, had to last in a mixture of known composition before ignition occurred.

The results which were obtained appeared at first puzzling. It was found that either the explosion occurred as soon as the spark passed at all or that it did not occur, however long the spark lasted; in a given mixture, which of the two alternatives occurred seemed to depend entirely on the temperature of the filament of the valve. If the temperature was gradually increased, the increase over a considerable range made no difference at all; however long the spark lasted—even if it lasted many seconds—no explosion occurred or any gradual combination, which would be shown by a decrease in the pressure of the mixture. But as soon as a certain limit was overstepped, the explosion occurred at the very first appearance of the spark and when it could not have lasted as long as 10 micro-seconds. The exact point at which the sudden change occurred varied considerably at successive trials, and varied also with the composition of the mixture, but the change was always perfectly sudden; no evidence whatever could be obtained that a discharge which would not cause explosion at the very first instant could cause explosion if it were maintained for any finite time.

The conclusion that ignition was so intimately connected with the starting of the discharge and that the discharge, if it did not cause ignition in the initial stages, would not cause it at all was not, perhaps, very surprising; several plausible explanations based on the process of ionisation, might be suggested. But it was very difficult to understand why, if the initial process of the discharge was all that mattered, the current flowing through the valve should make any difference. However, it is not worth while here to discuss all the speculations which were indulged in and all the fruitless experiments to which they gave rise, for the explanation was finally found to be so simple that it is now surprising that it was not expected from the outset.

Discontinuity of the Discharge.

The explanation was found as soon as systematic measurements were made of the quantity of electricity which passed through the spark gap to the electrometer. Two series of such measurements are shown in Fig. 2. The abscissæ are the time intervals corresponding to readings of the micrometer screw

on the pendulum ; one of these readings is arbitrarily selected as zero and the abscissa corresponding to any other reading is the time interval corresponding to the difference between that reading and the zero reading. The ordinates represent the quantity of electricity received in C_2.

Consider, first, the full-line in the figure. For values of the abscissa less than that represented by the point P no electricity passes, because K_2 is operating before K_1; P is the moment at which K_2 operates just after K_1, and times reckoned from P as zero are the times during which the high potential is applied to the spark gap.

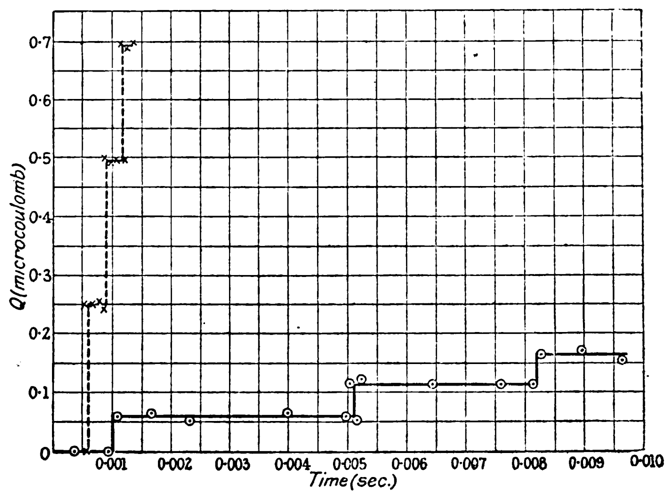

FIG. 2.

It will be seen that the quantity of electricity which passes across the gap does not increase continuously with the time that the potential acts; the flow of electricity is discontinuous. A finite quantity passes at the moment P in a time so short that it cannot be measured by the pendulum ; an interval elapses during which no electricity passes, terminated by the passage once more of a finite quantity in a time which is inappreciable.

The significance of these observations is obvious. They show that the passage of the discharge in the circumstances of these experiments is the same as that which takes place when a Leyden jar with a spark gap in parallel is charged by a Wimshurst machine. The machine takes a finite time to charge the

jar up to the spark potential; a spark then passes in a time which appears infinitesimal to the eye; an interval then occurs in which the machine charges up the jar once more, and the whole process is repeated. Exactly the same cycle is happening in the conditions described, but the interval between successive sparks is too small for the eye to detect.

And now the fact that if the mixture is not exploded by the discharge at the first instance it is not exploded if the discharge is continued, is explained at once; it is merely evidence that if one of the series of sparks which constitutes the discharge is not able to explode the mixture, a succession of them at intervals long compared with the time that each lasts is not sufficient to cause ignition; the effects of one spark have died away before the next occurs.

It is not so immediately evident why a larger current through the valve should produce an explosion when a smaller current fails to do so; for at first sight it might be expected that the only effect of increasing the current would be to increase the rate at which the sparks pass; while, if the explosion occurs at the first spark, if it occurs at all, the rate of succession of sparks after the first should be immaterial. However, the dotted line in Fig. 2 shows that another factor comes into play. The measurements shown by this line was taken when the current which the valve would pass was about 50 times as great as in the previous observations. It will be seen that the interval between the separate sparks (*i.e.*, the successive steps in the line) is considerably reduced, but it is not reduced in the ratio of the currents; the successive steps in the ordinates are much greater with the larger current. The effect of increasing the current is to increase the quantity of electricity which passes in each spark as well as the rate at which the sparks pass. Since sparks in which different quantities of electricity pass may well have different igniting power, it is not surprising that the larger current should prove to have the greater igniting power.

But why is the quantity of electricity which passes in a single spark greater when the current through the valve is greater? To answer this question we must know what determines the quantity of electricity which passes. In the case of the Leyden jar charged by the Wimshurst, this quantity, Q, is (at least, very nearly) CV, where C is the capacity of the jar and V the spark potential. For the spark starts when the potential across the jar is V, and when it is over the potential is very

nearly 0; the discharge takes so short a time that the quantity of electricity supplied by the Wimshurst during that interval is quite inappreciable. But the relation $Q=CV$ will hold only so long as the quantity of electricity supplied by the Wimshurst during the time that the spark lasts is inappreciable compared with CV. If there were substituted for the Wimshurst some source of current which could supply electricity at a rate comparable with that at which it crosses the gap in the spark, then Q would doubtless be considerably greater than CV; indeed, if the rate of supply were so great that it was actually greater than the rate at which electricity could flow across the gap, we should expect the whole phenomenon to be changed, and the successive discontinuous sparks to be merged together to form one continuous flow.

Now, the valve T_2 limits the rate at which electricity can flow to the spark gap. We may imagine that what happens at the first moment of the discharge is that the electricity which has accumulated in the condenser formed by the portions of the apparatus between T_2 and the spark gap passes across the gap in a single rush occupying an inappreciable time. If the greatest current which can pass through the valve is so small that during this time the quantity of electricity which the valve can pass is small compared with that present before the discharge started, then the conditions will be precisely the same as in the case of the Wimshurst and Leyden jar. The discharge will stop when the quantity $Q=CV$ has passed, where V is the spark potential and C the capacity of that part of the circuit which lies between T_2 and the spark gap. If, on the other hand, the rate at which electricity can pass through the valve is so great that during the very short time occupied by the spark, a quantity of electricity can pass to the gap which is not small compared with CV, then we should expect Q to be greater than CV. (*See* p. 191.)

In this manner we can explain qualitatively the effect of the filament temperature of T_2 (determining the greatest current which can pass through the valve) in determining the value of Q, the quantity of electricity which passes in a single spark, and consequently, the igniting power of that spark. In order that the whole matter should be adequately cleared up, it is essential that quantitative measurements should be found to be in accord with the theory suggested. A complete proof would be provided if it could be shown that, so long as the current through the valve were sufficiently small, Q is actually equal

to CV, where V is the spark potential and C the capacity of the circuit to earth between the valve and the spark gap ; and further, that i_0, the value of the current through the valve at which Q becomes greater than CV, is such that i_0T is comparable with CV, where T is the duration of the spark. But neither of these experiments is easy to perform with the apparatus unaltered ; for one thing it is very difficult to estimate at all accurately the capacity of such a complicated circuit as that which lies between T_2 and the spark gap. But in one respect such measurements as could be made were in accordance with the theory. Thus, it was found, as the theory predicts, that so long as the current was less than a certain value, Q was independent of the current. On the other hand, the value of the current at which Q began to increase was considerably less than that which would be expected, if for T is taken the maximum value fixed by the fact that observations such as those of Fig. 2 show that this time is inappreciable by the pendulum. More will be said of this discrepancy later.

5. *The Measurement of Q.*

If the explanation suggested is correct, any other device which prevents the current passing through the spark gap rising above the necessary limit during the passage of the spark should act in the same manner as the valve, and should limit the portion of the circuit which discharges through the spark to that between the limiting device and the spark gap. Accordingly, the effect was tried of inserting between T_2 and the spark gap a high resistance R in the position shown in Fig. 1. If the resistance of R is sufficiently great the value of Q when the spark passes should be determined by the capacity of the portion of the circuit between R and the spark gap, but should be independent of the capacity of the portion between R and T_2. This expectation was found to be fulfilled. If a condenser was inserted in parallel with the spark gap in the position C_3 of Fig. 1, then Q was increased whatever the value of R ; but if the condenser were inserted between R and T_2, then, if R were sufficiently great, its presence had no effect upon Q. The limiting value of R, such that it was just great enough to prevent a condenser inserted between T_2 and R affecting Q, was found to be of the oder of 10 megohms ; it would doubtless vary considerably with the exact conditions of the experiment, with the spark potential and possibly

with the form of the electrodes or the nature of the gas in which the spark passes.

The high resistance R was a small glass tube filled with a mixture of xylol and alcohol, and provided with platinum electrodes. The capacity of the resistance itself was less than 1 mmf. (1 mmf. $=10^{-12}$ farad), and small compared with that of the remainder of the spark gap circuit. Accordingly there was now no difficulty in determining the capacity of the portion of the circuit which discharges in the spark, for the connection between R and the spark gap could be broken, and the discharging portion completely insulated without any material change in its capacity. The capacity, in the absence of any added condenser, was determined by a simple modification of the ordinary method of mixtures; the circuit was charged to 500 volts by a battery, and then, by means of two

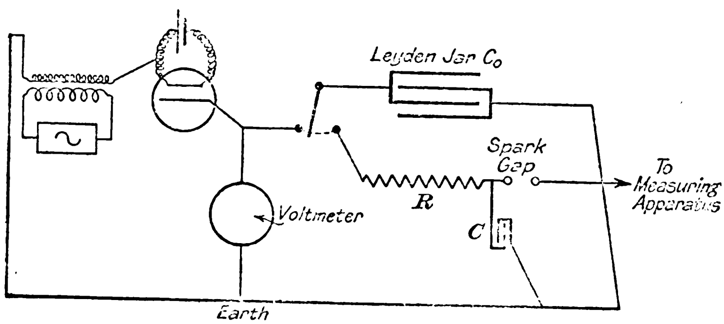

FIG. 3.

contacts worked at an interval of about 0·001 second by means of the pendulum, it was successively insulated and connected to a condenser of known capacity with the electrometer in parallel. The capacity of the discharging circuit could be increased by inserting condensers of known capacity in the position C_3.

A modification was also made in the method by which the number of sparks which pass across the gap was limited. In the arrangement shown in Fig. 1, the limitation is effected by means of the pendulum, which limits the time during which the high potential is applied to the spark gap; in the new arrangement it is effected by limiting the total quantity of electricity which is available. The connections are shown in Fig. 3. The Leyden jar C_0 is first charged up to any desired

potential, indicated by the voltmeter, by means of the transformer acting through the valve; by means of the throw-over switch it is then connected across the resistance R and the spark gap in series. If V is the spark potential of the gap, C_0 the capacity of the Leyden jar, C that of the spark gap circuit, then, if the Leyden jar is initially charged to a potential $V+v$, the potential across the gap, after the switch has been thrown over, will be $C_0(V+v)/(C_0+C)$. If this is greater than V, sparks will pass across the gap, each carrying away the quantity of electricity $Q=CV$, until the potential across the gap has fallen to less than V. If n is the number of sparks that pass, n is the integer next less than $C_0v/CV-1$. Accordingly by adjusting v, n can be given any desired value.

The first measurements which were made were directed to determine how far it was accurately true that $Q=CV$, where Q is the quantity of electricity which passes across the gap in a single spark, V the spark potential, C the capacity of the spark gap and any condensers in parallel—*i.e.*, of that part of the circuit which is effectually isolated from the remainder during the passage of the discharge by the high resistance R.

The results of these measurements are given in Table I. The value of V at the head of each section is the spark potential of the gap determined in the ordinary manner with a steady potential. The first column gives the measured value of C, the second the product CV, the third the measured value of Q. If $Q=CV$, the numbers in the second and third columns should be the same. However, since great care was not taken to adjust v so that n was always 1, sometimes two or three sparks may have passed and not only one; hence, it is to be expected that the numbers in the third column should be nCV, where n is usually 1, but may be 2 or 3.

It will be seen that in the first section of the table, the agreement between the second and third columns is remarkably good; differences in almost all cases lie well within the anticipated error of observation. The agreement persists in the earlier parts of the two later sections, but in these sections, when C is increased beyond a certain limit (less for the greater spark potential), numbers appear in the third column which are markedly different from those in the second; in some cases they are actually different in sign. These discrepant observations are shown in italics.

The occurrence of observed values of Q with a reverse sign is not very easy to understand. It is not surprising that

there should be, at some stages of the discharge, a current across the gap opposite in direction to that determined by the initial field; for it is to be expected that the discharge

TABLE I.

V=3100 volts.

C.	CV.	Q.
($\mu\mu f$.)		Microcoulombs.
16·0	0·050	2×0·052, 0·050, 2×0·053, 0·056.
21·2	0·066	0·072, 0·072, 0·069.
24·4	0·076	0·084, 0·087, 0·081.
32·6	0·101	0·106, 0·103, 0·109.
46·8	0·145	0·150, 0·144, 0·147.
71·8	0·223	0·228, 0·230, 0·228.
121·0	0·376	2×0·374, 2×0·348, 0·396, 0·396, 2×0·404.
177·0	0·550	2×0·503, 0·561, 0·561.
225·0	0·700	0·692, 0·673, 0·702.
346·0	1·072	1·04, 1·07, 1·06.

V=4230 volts.

C.	CV.	Q.
($\mu\mu f$.)		Microcoulombs.
16·0	0·068	0·069, 2×0·069, 0·065.
21·2	0·090	0·090, 0·090, 0·087.
24·4	0·103	0·103, 0·103, 0·100.
32·6	0·137	0·134, 0·134, 0·137.
46·8	0·197	0·187, 0·184, 0·187.
71·8	0·303	0·303, 0·294, 3×0·286, 0·294, 0·296.
88·4	0·372	3×0·323, 0·372, 3×0·320, 0·372.
103·0	0·435	1·22 (=3×0·407 ?), *0·694*, *2·10*.
121·0	0·511	0·538, 0·519, *−0·121*, 0·528, *0·728*, 1·50. (=3×0·50 ?).
138·0	0·583	0·607, *4·23*, *−0·719*, *3·62*, *0·820*, *0·017*.

V=5350 volts.

C.	CV.	Q.
($\mu\mu f$.)		Microcoulombs.
16·0	0·086	2×0·086, 0·090, 0·094, 2×0·087, 0·094.
21·2	0·114	0·118, 2×0·115, 0·118.
24·4	0·131	0·137, 0·134, 0·134.
32·6	0·175	0·172, 0·168, 0·166.
46·8	0·251	0·256, *0·340*, *0·106*, *0·066*, *0·375*.
60·7	0·332	*−0·451*, *0·107*, *2·17*.

should be oscillatory. But, if current flows nowhere except between the two poles of the spark gap, a charge left finally on the low potential side of the gap opposite in sign to that

originally present on the high potential side must indicate that the high potential side has increased its potential, and that the final field across the gap is greater than the initial field. Since this conclusion must be erroneous, the assumption on which it is based must be wrong, and current must flow elsewhere than between the two poles of the gap; for instance, it may flow from the high potential terminal to earthed conductors in the neighbourhood of the gap. And such an occurrence is not impossible, for the "explosive action" of the spark may carry some of the ions to a considerable distance, so that they lose their charge on conductors other than the terminals of the gap. If part of the charge originally present on the high potential terminal is thus given to other conductors, a charge of reversed sign left finally on the low potential side of the gap might be explained. It should be observed, therefore, that the discrepant observations occur when the energy dissipated in the gap is increased by increasing either the spark potential or the capacity discharging; the greater the energy dissipated, the greater is the explosive action of the spark, and the more likely that part of the charge originally present is carried to conductors other than the terminals of the gap.

It is obvious that this view might have been tested by experiment, but since the primary object of the research was not to investigate in detail all the processes occurring in the spark, but only those which are likely to determine its power of igniting explosive mixtures, further investigation in this direction was not undertaken. The experiments sufficed to show that under certain conditions the quantity of electricity which passes across the gap is simply the product of the spark potential and the capacity discharging. This conclusion, which, of course, might have been anticipated from the start, was all that appeared necessary for the later experiments.

But one further observation should be noted. In all these experiments Q has been measured for the first spark which passes. It is interesting to inquire whether, if the first spark is followed by a stream of others in rapid succession, the later sparks are characterised by the same value of Q as the first. It was found that Q was in general less for the later sparks, if they followed each other sufficiently rapidly. Some indication that Q is smaller for the second and succeeding sparks may be obtained from Table I., but more definite evidence is shown by Fig. 2. It will be seen there that the

second and succeeding steps in the line are smaller than the first. The explanation of this difference probably lies in the fact that V, the spark potential, is less for the second spark than for the first. It is well known that the potential necessary to maintain a spark across a given gap is somewhat less than that needed to start it; the difference is probably due, in part at least, to the decrease in the density of the gas surrounding the spark gap by the passage of the first spark.

6. *The Duration of the Spark.*

Attempts were made to determine the duration of the spark, but they led to no definite result, and gave only an upper limit to the possible duration.

It was thought that if the contact K_2, which short-circuits the spark gap, could be closed during the actual passage of the spark, the discharge across the gap would be stopped, and the remainder of the charge on the high potential side diverted to earth through the contact, the quantity of electricity received by the low potential side of the gap would then be less than the normal amount. However, all attempts to obtain by variation of the interval between the operations of the contacts K_1 and K_2, values of Q which were less than the normal amount proved fruitless; either no spark passed at all, and Q was 0, or a spark passed and the full value of Q equal to CV was obtained. No intermediate value was recorded even when the conditions were such that a change in the interval between the operations of the two contacts of less than 0·00001 second produced consistently a change from $Q=0$ to $Q=CV$. At first sight these observations would seem to show that the time occupied by the spark must be very small compared with 0·00001 second. It is quite possible that this conclusion is correct, but further consideration shows that it is not established by the experiments. However small the time occupied by the discharge, it would be expected that in a large number of experiments (and over 300 were made in this connection) chance would decide that in one of them K_2 would operate during that time; the failure ever to observe a value of Q intermediate between 0 and CV suggests that it is impossible by the operation of the switches to stop the spark once it has started discharge across the gap. And further consideration provides an explanation of the impossibility. When K_2 closes, it does so first by means of a spark across the approaching contacts. Sup-

pose, then, that the interval between the operation of K_1 and K_2 is such that K_2 sparks just before the spark potential of the main gap is reached, so that no spark passes across that gap; then the distance between the contacts of K_2 when the spark passes will be just less than that corresponding to that spark potential. If the action of K_2 is now delayed very slightly, the spark will have started across the main gap before K_1 reaches the position in which it previously sparked; owing to the passage of the spark across the main gap the potential will have fallen, and no spark will pass across the contacts of K_2 in this position. Further, no spark will pass at all across the contacts of K_2 unless the approach of those contacts is so rapid that the diminution of the spark potential between them is more rapid than the fall of potential across the main gap due to the passage of the discharge. Accordingly the failure ever to stop the spark once it has started by the operation of K_2 only shows that the rate of approach of the contacts was not sufficiently rapid. The known rate of approach and the variation of the spark potential with the separation of the contacts admits of a rough maximum estimate being made of the duration of the discharge; this maximum estimate turns out to be about 0·00005 second.

An attempt was also made to perform the same experiment in a rather different way. In place of using the second contact of the pendulum to short-circuit the spark gap, it was used to break connection at K_3 (Fig. 1) between the low potential side of the gap and the measuring apparatus. Again, it would seem that if such a break could be effected during the actual passage of the spark a value of Q would be recorded greater than 0, but less than the normal value. But this attempt also failed; values of Q other than 0 and CV could be recorded, but they were wholly irregular, sometimes greater than CV and sometimes of the reverse sign. The anomalies were traced partly to the fact that the charge received by the measuring apparatus requires a finite time to reach its ultimate distribution among the various parts of the apparatus; this time is apparently of the same order as that occupied by the discharge. But it was also suspected that, if the break were made during the actual progress of the spark across the main gap, a discharge took place across the separating contacts; for it must be remembered that, though the whole quantity of electricity transferred is very small, the current which flows during the actual time of passage may be considerable.

Accordingly, direct measurements of the duration of the spark gave no definite result; an indirect estimate yielded no more satisfactory information. The explanation which has been put forward for the effect of the high resistance R in limiting the quantity of electricity which passes in the single spark indicates that the value of R must be so great that the quantity of electricity which passes through it, while the sparks lasts, must be small compared with that accumulated on the high potential terminal before the discharge begins. This condition involves that CR, where C is the capacity which discharges, shall be large compared with T, the duration of the spark. Now, when C was 25 mmf., it was found that R had to be greater than 10 megohms, in order that the necessary condition should be fulfilled. CR is thus 0·00025 second, and T should be less than this value. The previous conclusion was that T could not be greater than 0·00005 second, or one-fifth of CR. There is thus no certain conflict between the two estimates, but on the other hand, since the previous estimate is nothing but a maximum estimate, and may exceed very greatly the true value, it can hardly be said that the indirect method confirms the direct. It may be pointed out in this connection that it is not certain that the T, which must be small compared with CR, is the same quantity as that which it was attempted to measure directly. The latter is the time during which current actually flows across the gap; the former is the time after which another rise of potential will not produce any current until the spark potential is reached once more. Now it is quite possible that, even after the discharge has ceased, strong residual ionisation may remain between the terminals of the gap which will permit an appreciable transfer of electricity at potentials far less than that required to initiate the discharge.

It will be observed that, according to these considerations, the value of R necessary to limit the discharge to the portion of the circuit between the resistance and the spark gap should decrease as the capacity C increases, if the duration of the spark is independent of the capacity. A few rough observations certainly showed that the necessary value of R does decrease as C increases, but sufficient measurements were not made to obtain any information as to the probable variation of T with C.

The only conclusion, then, that can be certainly based on these experiments is that the spark does not last as long as

0·00005 second. But there is some reason to believe that this maximum estimate is considerably greater than the true value. For it seems probable that the duration of the very feeble sparks, discharging very small capacities, with which we are here concerned would be less rather than greater than the duration of strong sparks due to the discharge of large Leyden jars. Now a limit is set to the duration of such strong sparks by their use in illuminating rapidly moving objects; familiar illustrations—*e.g.*, of moving rifle bullets—shows that the light from such strong sparks can certainly not last as long as 0·00001 second. But it must be remembered that the period for which the light lasts is not necessarily identical with that during which an appreciable current flows. On the other hand, observations of a quite different character would seem to set a lower limit to the duration of such strong sparks. The effective resistance of such sparks has been estimated from the damping which they produce in oscillating circuits with frequencies of the order of 10^6. It would seem that, in order to obtain self-consistent measurements of the resistance by such a method, the spark must last for a time comparable with the period of the oscillations, and therefore that the duration cannot be much less than 0·000001 second. But, once more, it is very doubtful whether these estimates can be applied to the very much feebler sparks which with these observations were concerned.

7. *The Limits of the Discontinuous Discharge.*

It will be seen that the investigation diverged widely from the scheme that had been planned at the outset. It had been expected originally that it would be possible to obtain and control a continuous discharge of which the duration could be readily varied, but the only form of discharge that was found was discontinuous, consisting of a succession of individual and indivisible sparks, the duration of which could not even be measured, far less controlled. For the purpose for which the research was undertaken this failure of expectation was unimportant; the object was to investigate the igniting power of such discharges as are likely to occur in practice, and when it was known that all such discharges were discontinuous, the investigation of continuous discharges ceased to be of immediate interest. Nevertheless, it is well to consider briefly how far the results which have been attained

indicate that in any conditions a continuous discharge in air at atmospheric pressure is likely to be obtainable.

The experiments seem to show that a continuous discharge could only be obtained if the rate of supply of electricity to the spark gap could be made so rapid that, during the very short time for which the individual spark lasts, a quantity of electricity nearly equal to that passing in each spark flows to the gap. If this condition could be obtained, it is to be expected that the successive individual sparks would merge into each other, and some form of continuous discharge obtained. In order to fulfil this condition, one or two alterations in the circumstances of the experiment must be made; either the current passing to the spark gap must be increased, or the quantity of electricity passing in each spark must be decreased. It is interesting to inquire whether by making changes in either of these directions it is possible to obtain a discharge in which measurements similar to those shown in Fig. 2 will give a continuous line, indicating that the flow of electricity across the gap is continuous, and does not take place in a series of isolated sparks.

It is difficult to reduce very much further the value of Q, the quantity of electricity passing in each spark. For reasons which will be given in a later communication, it is scarcely possible to reduce V, the spark potential, below 2,000 volts, while C, the capacity, cannot easily be reduced below 10 mmf. The least value of Q which is possible for the apparatus as designed is therefore 2×10^{-8} coulomb. If T, the duration of the spark, is not greater than 0·00005 second, the current supplied to the gap must be at least 0·0004 ampere before fusion of successive sparks is to be expected. The value of R necessary to isolate the discharging capacity increases as Q decreases; with so small a Q, R would have to be 100 megohms. Now, to drive 0·0004 ampere through 100 megohms requires 40,000 volts; and such a potential would be quite impossible to control by the means hitherto adopted.

This difficulty might be avoided by "isolating" the discharging capacity by a valve instead of by the high resistance; the potential required with that arrangement is only slightly greater than the spark potential of the gap. But when the valve is employed it is necessary to insert the arrangement for controlling the time that the discharge lasts between the valve and the spark gap, and the capacity of that arrangement is included in that which discharges in the spark. Under these

circumstances it is not possible to reduce C below 60 mmf.; Q cannot be less than 12×10^{-8} coulomb, or the current less than 0·0024 ampere. But when so large a current is passed through the valve, the stage, mentioned on p. 180, is reached, at which the valve ceases to isolate effectively the part of the circuit between it and the spark gap. Although the discharge which still passes consists of individual sparks separated by intervals in which no current flows, the value of Q for those sparks is considerably greater than CV, and the stage at which the coalescence of successive sparks is to be expected is still further postponed.

For these reasons it was not found easy to follow experimentally the passage of the discontinuous into the continuous discharge; indeed, it was not certain that a continuous discharge could be obtained at all, until the stage was reached in which an arc occurs. It is clear that, if the current and, therefore, the energy dissipated in the discharge are increased sufficiently, a condition must ultimately be reached in which the electrodes will become sufficiently heated for the discharge to pass into the arc determined by thermionic emission from the kathode. Continuous discharges could certainly be produced by increasing the current passed by the valve, but it appeared that they were of the nature of arcs rather than true independent discharges.

There is another way in which a continuous in place of a discontinuous discharge can be obtained—namely, by a change in the form of the electrodes. In all the experiments described here the electrodes were such that the radii of curvatures of their surfaces were large compared with the sparking distance. If, on the other hand, the sparking distance is large compared with the radius of curvature of the electrodes, it is possible to get the brush discharge with a P.D. less than that required to give the discontinuous spark. It is probable that if more finely pointed electrodes had been used it would have been found that the coalescence of successive sparks into a continuous discharge would have occurred for smaller values of the current, and the process might have been observed experimentally. But experiments were not extended in this direction, and it was subsequently found that much of the work which might have been attempted had already been described in an interesting, though a somewhat diffuse, paper by Max Toepler ("Ann. d. Phys.," II., 7, 560-635, 1900). This writer has examined the conditions

which determine the passage of the discharge from the brush to the discontinuous spark, and thence to the arc, as the current is increased; and he found, in accordance with the indications of the work that has just been described, that the region in which the continuous discharge was possible was narrower the greater the capacity in parallel with the spark gap.

8. "*Capacity*" *and* "*Induction*" *Sparks.*

The observations that have been recorded show that the sparks which have been investigated were essentially similar to the "capacity" or "condenser" sparks of the introductory paragraphs produced by the discharge of a charged condenser. It is interesting to consider what bearing, if any, they have on the nature of the "induction" spark.

If the spark produced between the secondary terminals of an induction coil when a current is broken in the primary is observed in a rotating mirror or photographed on a rapidly moving film (*), it is found to consist of a succession of individual sparks separated by intervals in which no discharge is seen. The first spark is always much brighter than those which follow. The sparks after the first are separated by equal intervals, which correspond to time intervals equal to the period of the oscillation of the secondary circuit; the interval between the first spark and the remainder is not in general the same as that separating the later sparks.

The question arises whether these sparks, like those investigated hitherto, are characterised by the passage across the gap of a definite quantity of electricity, and, if so, by what this quantity is determined. On analogy with what has been found already we should expect that the first spark, at least, would convey a quantity of electricity Q, equal to the product of V, the spark potential, into some capacity C; and at first we should be inclined to identify this capacity with the capacity of the secondary circuit as determined by measurements of the oscillation frequencies.

Some investigations in this direction were begun, but were not completed, when more urgent problems claimed attention; nevertheless, the preliminary results were of sufficient interest

* The structure of the spark produced by a magneto, which, of course, is an "induction" spark, may be investigated very conveniently by means of the "rotary spark gap," which has been developed for investigating the timing of magneto sparks. The bright first spark, followed after a definite interval by much fainter sparks equally spaced, or sometimes by a continuous "flame," is very clearly seen.

to deserve record. In these experiments a current flowing through the primary of a small induction coil was broken by the first pendulum contact; the secondary terminals were connected to a spark gap which was short-circuited, as before, by the operation of the second contact. The low potential side of the gap was connected, as before, to the apparatus for measuring the quantity of electricity which had passed across the gap. By varying the interval between the operation of the pendulum contacts the quantity of electricity which passed during known intervals after the break of the primary circuit could be measured.

It was found, as was to be expected, that the relation between the interval between the operation of the contacts and the quantity of electricity received was of the same form as that shown in Fig. 2. As soon as a spark passed at all, a finite quantity of electricity was received in a time which could not be divided by the pendulum; then an interval occurred in which no further charge was received by the measuring apparatus. The quantity of electricity passing in the first spark was a perfectly definite quantity, and its value could be determined consistently at successive trials so long as the apparatus remained unchanged. After a certain interval after the first spark, a stage was reached at which a further quantity of electricity was received by the measuring apparatus, indicating the passage of a second spark of some kind, but the preliminary observations showed that the quantity passing in this subsequent discharge was much less regular than that passing in the first spark; consistent measurements could not be obtained in the conditions employed. Accordingly, attention was paid only to the first spark.

It was found immediately that the expectation that Q would be equal to CV, where C is the secondary capacity, was not fulfilled. In the first place, although the spark gap (and, therefore, presumably V) was unchanged, Q increased rapidly with the current broken in the primary. This conclusion might have been anticipated without any detailed measurements, for it is a familiar observation that the "fatness" or luminosity of the spark from an induction coil, the gap being unchanged, increases rapidly with the current broken in the primary. In general, a greater luminosity indicates that a greater quantity of electricity passes across the gap in the spark. Now, the secondary capacity of an induction coil is a

" distributed " capacity, and therefore a somewhat indefinite quantity, but there is no indication that it varies with the primary current; for the period of the oscillations excited (which is determined in part, by the secondary capacity) does not vary with that current. Accordingly, if we are to write $Q=CV$, the C in this equation is not the secondary capacity as estimated by oscillation methods.

Again, if $Q=CV$, Q should increase when the sparking distance and the sparking potential are increased; but

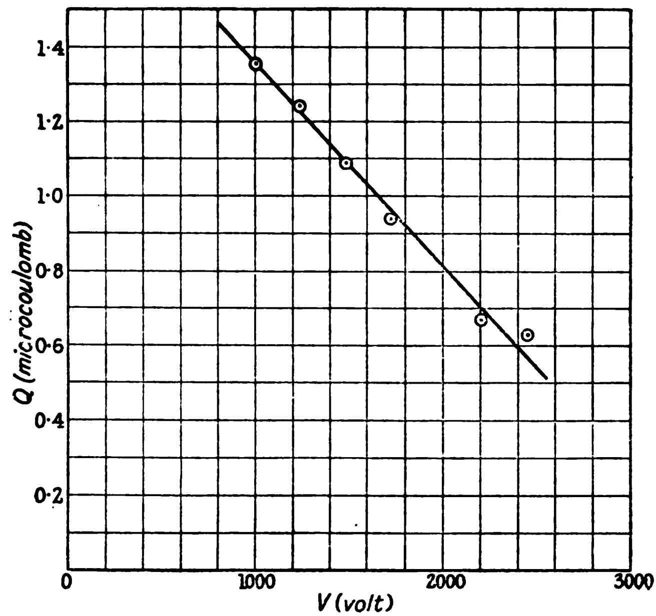

FIG. 4.

measurements show that it decreases. Fig. 4 gives the relation between Q and V in one series of experiments, in which the current was the same throughout. In no respect, therefore, is there even qualitative agreement between theory and experiment, if we make C a constant quantity and identify it with the normal secondary capacity of the coil. Nor is there quantitative agreement. The secondary capacity of the coil was not actually determined, but it is known that it could not have been as great as 30 mmf.; whereas the lowest value determined from the relations $Q=CV$ (namely, with the

greatest sparking distance across which a spark could be obtained with a current of 0·5 ampere) was 300 mmf.

It is clear then that the quantity of electricity which discharges in the induction spark is not simply determined by the capacity of the spark circuit; and further consideration shows that this conclusion is not surprising. For the normal secondary capacity applies only to the condition when the secondary terminals are insulated externally; when the spark starts they are joined by a conductor, and the presence of this conductor is equivalent, so far as determination of the period of the oscillations is concerned, to the addition of a condenser of large capacity. Indeed, the fact that the passage of the spark makes the secondary circuit for the time being a closed conducting circuit suggests that a quite different method of calculating Q is worthy of consideration. If, when a current, i, is broken in the primary, the secondary terminals are joined by a metallic conductor, so that the total secondary resistance is R, then, by a well-known theorem, the total quantity of electricity which will pass round the secondary circuit as a result of the break of the current in the primary is iN/R, where M is the mutual induction of the primary and secondary circuits. Now, the spark is such a conductor joining the terminals of the secondary, and, if we knew what value to attribute to its effective resistance we might calculate Q from the formula just given; or, conversely, we might use the formula to calculate the effective resistance of the spark. Unfortunately, however, it does not seem that this method is legitimate, for the Q which is equal to iN/R is the quantity which has passed round the circuit after a time so long that all disturbances excited by the break in the primary have died away; this time is certainly not less than the period of oscillation of the coupled circuits. But the spark does not last more than a fraction of that period. Nevertheless, though the formula cannot be applied strictly, it provides a qualitative explanation of some of the relations previously noted; thus, a decrease of the spark gap might be expected to produce a decrease of the effective resistance of the spark, and we actually find that it produces an increase of Q, and again Q increases with i.

The relation between Q and the characteristics of the gap and of the coil certainly deserves further consideration both from the theoretical and the experimental side. The results which have been given suffice to show that, in any given conditions, the spark produced by the induction coil, like that

produced by the discharge of a condenser, is characterised by a definite value of Q. This is the result which appeared important for the main purposes of the research. For if it be true that capacity and induction sparks are both characterised by a common feature, namely, that in both the passage of the spark is accompanied by the passage of a definite quantity of electricity across the gap, the relation which it was hoped to establish between the two forms of discharge has been found. It is to be expected that if the electrodes and the sparking distance are the same, the igniting power of the spark will be determined by Q; and that two sparks, produced one by an induction coil and the other by the discharge of a condenser, will have the same igniting power if for both of them the value of Q is the same. Of course, experiment will be necessary to establish that proposition; it is still possible that there might be a difference, as, for example, in the time that the spark lasted, which would make a difference in the igniting power, even if Q were the same.

Summary.

1. The object of the investigation was to determine the relation between the electrical characteristic of a spark discharge, and its power of igniting explosive mixtures.

2. Previous work on the subject is briefly reviewed. Certain general conclusions appear to have been established, and in particular it has been shown that the energy dissipated in the discharge is not the factor of prime importance in determining its igniting power. But most of the work relates to igniting power to the properties of the apparatus by which the discharge was produced rather than to the properties of the discharge itself.

3. The scheme of the research is explained. An attempt was made to produce a form of discharge in which the current passing and the time for which it lasted could be controlled and varied.

4. Preliminary observations show that the attempt to obtain such a discharge had failed. The discharges obtained always consisted of a discontinuous series of individual sparks, each of which lasted for a time which could not be sub-divided.

5. Quantitative measurements show that each of these individual sparks consists in the passage of a definite quantity of electricity Q across the gap, and represents the discharge

of a condenser of definite capacity previously charged to the spark potential of the gap.

6. Attempts to determine the duration of each of the individual sparks only led to a maximum value being assigned to that duration. This maximum is 0·00005 second; there is reason to believe that the true value is considerably less than this maximum estimates.

7. The limits to the conditions in which the discharge will be discontinuous and of this form are considered. It was not found possible experimentally to obtain a discharge which was continuous, except when it took the form of an arc or a brush.

8. Some preliminary observations on discharges produced by an induction coil are described. It is shown that such sparks, like those obtained by the discharge of a condenser, are characterised by the passage across the gap of a definite quantity of electricity. It is suggested, therefore, that this quantity, together with the form of the gap, may be sufficient to define the nature of the spark and to determine its igniting power.

The second part of the Paper will deal with observations on the igniting power of spark discharges.

PART II.—IGNITING POWER OF SPARK DISCHARGE.

9. *Plan of the Research.*

It has been established in the preceding part of this Paper that the true spark discharge is discontinuous and consists of a succession of individual sparks; each of these sparks lasts a time so short that it has been found impossible to measure it, and each is associated with the passage across the spark gap of a definite quantity of electricity determined on the one hand by the spark potential, on the other by the nature of the source of potential and of that part of the circuit which connects the source to the spark gap. The individual sparks are essentially similar to the spark which occurs when a condenser is raised to the spark potential of the gap and then allowed to discharge across it; but the capacity of the condenser which gives the equivalent spark in such circumstances is determined by the characteristics of the remainder of the circuit.

The "capacity" spark, then, seems to represent the normal type of spark, and sources of supply which appear essentially different from that by which the capacity spark is usually produced give discharges which differ from the capacity spark only in the fact that they give a succession of sparks (which may or may not all be similar) in place of a single spark. If this conclusion is correct it follows that, if we investigate thoroughly the igniting power of capacity sparks, we shall be able to predict the igniting power of any form of spark by whatever agency it is produced. A considerable amount of information concerning ignition by a capacity spark, produced directly by the discharge of a condenser in the ordinary way, is already available from the work of Thornton. but his observations were limited to those in which the capacity of the condenser discharged was large (more than 1 mf.) and the spark potential small (a few hundred volts). The extension to smaller capacities, and therefore, if ignition is to ensue, larger spark potentials would have proved difficult, if no way of producing a capacity spark had been available other than that of first charging a condenser to the requisite potential and then approaching its terminals; by such a method it would obviously have been difficult to extend the observations to capacities as small as a few micro-microfarads. But the experimental arrangements described in § 5 enabled such small capacities as these to be used with ease; we have only to isolate the small capacity from the rest of the circuit by means of a sufficiently high resistance and then apply a steady potential; a stream of sparks will then flow, each representing the discharge across the gap of the small capacity charged up to the spark potential.

Whether or no a given spark will ignite a given mixture will probably depend on the following characteristics:—

(1) The nature of the mixture, *i.e.*, its chemical composition, pressure, temperature, and, possibly, state of motion and electrical characteristics. In what follows, unless the contrary is stated, mixtures which differ in any of these respects will be regarded as different mixtures, *e.g.*, mixtures of the same composition at different pressures.

(2) The capacity which discharges across the gap in the spark.

(3) The nature of the gap—*i.e.*, the form and material of the electrodes and the distance between them. These characteristics determine the spark potential in conjunction

with (1); the spark potential in conjunction with (2) determines Q, which is the quantity of electricity which passes across the gap in the single spark.

In the experiments about to be described little attention is paid to (1). Mixtures of various natures have been investigated, but the variation has been determined either by experimental convenience or selected as a means to investigating the effect of (2) and (3). In all the experiments (2) has been varied within wide limits; in those of the first series the form of the electrodes was unchanged, while the spark potential was varied by changing the distance between them. In a later series the effect of the form and material of the electrodes is considered. The main object has been to discover laws relating the electrical characteristics of the discharge to its igniting power which shall be generally applicable to all mixtures.

10. *Preliminary Experiments.*

But before entering on an account of the main experiments one possibility which has been left unnoticed must receive attention. It has been tacitly assumed that the igniting power of a discharge is determined wholly by that of the individual sparks, of which it consists; but it is clearly possible that a mixture might be ignited by a rapid stream of similar sparks when a single spark of the same nature could not cause ignition. The experiments already described in §4 provide evidence against such a supposition, for it will be remembered that it was found that if a discharge did not ignite the mixture when the first spark passed it would not ignite it, however long the discharge was continued. However, as the matter is very important, a preliminary series of experiments was undertaken to settle the matter.

One possible source of difficulty should be mentioned. It has been noted by all observers on ignition by the electric discharge that it is impossible to obtain perfectly consistent results. It is often found that a discharge which will ignite a mixture at one trial will fail to ignite it when the experiment is repeated in circumstances apparently exactly similar; the critical intensity for ignition has always to be taken as that which will explode the mixture in some definite proportion of trials.

Such inconsistency might make it hard to determine with certainty whether an increase in the number of similar sparks

passing in succession really increased the igniting power the discharge. For if we found that ignition occurred more often when two sparks were passed in rapid succession than when only one spark passed, we might be doubtful whether the increase in frequency was due to the passage of the first spark aiding ignition by the second, or simply to the fact that double the number of single sparks passed in a given number of trials, so that the chance of one of them causing ignition was increased. The doubt could only be settled by a somewhat elaborate investigation of the frequency with which discharges consisting of different numbers of successive sparks caused ignition; such a statistical inquiry would be very laborious, and its results would always be open to some doubt.

Fortunately, however, it was not necessary to consider these complications, for no consistent increase whatever could be found in the frequency with which a given discharge caused ignition as the number of individual sparks passing in succession was increased. A single spark followed by no others caused ignition as frequently as two, three or four sparks passing in rapid succession. The arrangement described in § 5, where the number of sparks passing is limited by the quantity of electricity supplied, provided a convenient means for trying the experiment; in order to vary the number of sparks which passed it was only necessary to vary the initial potential of the condenser C_0. The interval between successive sparks obtained by this method is also as short as can be readily obtained; it was not measured directly, but was certainly less than the estimated maximum duration of a single spark and probably did not exceed 0·00001 sec.

It is permissible, then, to conclude that if a single spark will not explode a mixture, a succession of a small number of similar sparks in a very rapid succession will not explode it, and therefore that the igniting power of any discharge is simply that of the individual sparks of which it consists. Further, since the frequency of ignition does not increase at all with an increase of the number of sparks from one to four, it is clear that the condition of which the variation causes the inconsistency in successive trials must be such that it does not change in the small interval which separates successive individual sparks in the same discharge. But a limitation to the conclusion must be noted. It has already been explained that the individual sparks which follow the first are

not entirely similar to the first; they seem to carry a rather smaller quantity of electricity and probably pass at a lower spark potential. We shall see presently that the igniting power of a spark decreases rapidly with the spark potential; accordingly the fact that the mixture always explodes, if at all, at the first spark may only be due to the passage of the succeeding sparks at a lower spark potential. Since, however, this difference between the first and succeeding sparks is likely to be present in all forms of the discharge, the practical conclusion that the igniting power of the discharge is determined wholly by that of the first individual spark remains unaltered.

Again, it appears necessary to insist that in the application of the proposition the limitation to "a small number" of successive sparks be maintained. Cases were occasionally observed in which, though the mixture did not ignite at the first spark, it did ignite when the discharge had been continued for some seconds or even minutes and many thousands of sparks had passed. It is thought probable that such observations, which were very irregular, are to be attributed to the inconsistency which affects all the observations; the mixture exploded after a long time because in the interval since the first spark the variable condition which in part determines the ignition had changed; the mixture had simply passed into a more easily explodable condition.

After these preliminary remarks we can proceed to consider the main experiments.

11. *The Main Experiments: The Apparatus.*

Since the work was originally undertaken with a view to its application to petrol engines, it was thought desirable to make the main experiments on mixtures of petrol and air, although the exact conditions of the engine cylinder could not be reproduced, while the use of a liquid fuel introduced many manipulative difficulties. The chief of these difficulties arose in the filling of the explosion vessel with a mixture of known composition and from the necessity of maintaining it at a temperature of 50°C. in order to secure complete evaporation of the petrol.

It was expected from the outset that trouble would be found from the source of inconsistency already discussed; and every effort was therefore made to render the conditions of experiment capable of exact reproduction. The plan of

using as the explosion chamber some kind of engine cylinder, of which the filling and evacuation would be effected automatically, was rejected in favour of an apparatus in which the filling could be controlled more accurately, although much extra labour was involved. It may be said at once that the expected greater consistency hoped for was not realised, and that it would probably have been better to have adopted the less laborious method of operation.

A drawing of the explosion chamber used is given in Fig. 5, which needs little explanation. The spark gap is between steel

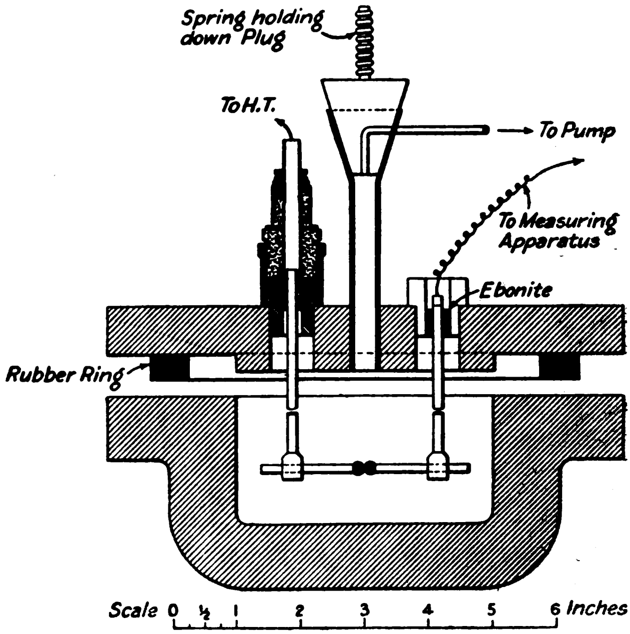

FIG. 5.—EXPLOSION CHAMBER.

balls $\frac{3}{16}$ inch in diameter, the distance of which is adjustable. The low potential ball, connected to the measuring apparatus, is carried on an insulated support made from a Lodge sparking plug with mica insulation; but, since it is important to keep the capacity to earth of the high potential electrode as small as possible, the support for the other terminal was specially constructed with this object in view.

The whole chamber was immersed in a water bath kept within 1° of 50°C., the water level being about $\frac{1}{2}$ in. above the

upper surface of the plate which supports the electrodes. The tap, projecting above that level, was kept hot by means of a small flame.

The petrol was injected directly into the chamber by removing the plug of the tap and passing a graduated pipette into the tube connecting the tap and the chamber. The plug was then inserted and screwed down ; air was introduced along the tube by means of a motor-tyre foot-pump until the desired pressure was read on the gauge, which had been calibrated. The tap was then turned to isolate the contents of the chamber, which was then sufficiently air-tight not to lose more than 5 per cent. of its excess pressure in half an hour. The observations on the ignition of a single filling never occupied more than 5 minutes, so that the leak during this period was inappreciable.

For the purpose of stating the composition of the mixture in the chamber, the quantity p will be used ; p is the ratio of the weight of the petrol injected to the weight of the air which would occupy the chamber at the same temperature and total pressure ; p is therefore not equal to, but rather less than, the ratio of the weight of petrol to the weight of air in the mixture, for some part of the total pressure was due to the petrol vapour and not to the air. For an exact knowledge of the relation between p and the weight-ratio of petrol/air, information concerning the effective molecular weight of the petrol would be required ; such information is not sought, for the values of p given are used only to identify explosive mixtures of given composition and no use of the values is made for stating quantitative conclusions. The only assumption made in the course of reaching the conclusions to be given is that mixtures for which p is the same have the same chemical composition, whatever the total pressure of the mixture. This assumption is true if all the petrol is present in the state of vapour at all pressures ; and if, further, the effective molecular weight of the petrol does not vary greatly with the total pressure (or, in other words, that there is no very great variation in the departure from Henry's law). The best proof of the truth of the assumption, at least in the degree necessary for this work, is that the value of p for the most easily exploded mixture was actully found to be independent of the total pressure ; for it is probable that the chemical composition of the most easily exploded mixture is nearly independent of the pressure.

After the ignition experiments had been conducted and whether or no an explosion had occurred, the plug of the tap was removed again and a tube, connected to a Fleuss air-pump and calcium chloride vessel, inserted into the opening of the tube. The fit was tight enough for the pressure to be reduced to a few millimetres of mercury. The evacuation served to clean out the vessel and also to restore the insulation of the electrodes which had often been spoilt by the deposition of water. The cycle of filling and evacuating the vessel took 3½ minutes, a fact which explains why the number of observations is so limited.

For the regulation of the discharge and the obtaining of a definite number of single sparks the method § 5 was employed.

A high resistance R (from 15 to 200 megohms) was inserted between the switch and the gap, in order to limit the discharging capacity to that on the low potential side of this resistance, as explained in § 5. The capacity of the gap with its connections to R was 16·0 mmf.; and this was the minimum capacity with which observations could be made. The capacity in parallel with the spark gap could be increased by adding condensers of known capacity; the values of these capacities were chosen so that the total capacity could be varied by steps of between 2 mmf. and 3 mmf. between the limits of 16 and 930 mmf. The smaller condensers added were made of rubber cable with an exterior coating of tinfoil, the larger of iron rods covered with micanite on which was wrapped a similar coating. The insulation resistance of the whole system of condensers and spark gap was never less than 10^{11} ohms and usually greater than 10^{12} ohms.

12. *Course of the Observations.*

In taking observations the spark gap was first fixed and the explosion chamber immersed in the water bath. A series of observations on explosions were then taken in which the total pressure of the gas in the chamber, measured by the pressure gauge, was constant, and the proportions of the mixture altered by varying the amount of petrol injected into the chamber. In each observation, after the chamber had been filled, a series of single sparks was sent through the mixture, while the capacity in parallel with the gap was increased in steps.

The inconsistency of consecutive trials was so great that it was unnecessary to increase C by the smaller steps possible

except at the lowest values. The following series of values of C was adopted in most of the work :—

16·0, 21·2, 24·4, 32·6, 46·8, 71·8, 121, 177, 225, 272, 346, 388, 426, 478, 542, 599, 701, 808, 916 mmf.

At each value of the capacity 10 trials were made, unless the mixture exploded before the 10 were completed. In most cases, of course, explosion occurred (if at all) at some trial between the first and the tenth. In order to give a numerical value for the critical capacity, the following rule was adopted : If the mixture had failed to explode in 10 trials with the capacity C_1 and exploded at the mth trial with the next larger capacity C_2, then the critical capacity was taken as equal to

$$\frac{(10-m)C_1+mC_2}{10}.$$

No theoretical justification whatever is claimed for exactly this procedure ; but it probably served as well as any other for fixing definitely a numerical value which could be used in the comparison of results.

When the ignition of a sufficient number of mixtures of different compositions had been investigated at one given value of the total pressure of the gas, a similar series was taken for each of several other values of that pressure. And when such series at different pressures had been completed, the length of the spark gap was changed and the whole procedure repeated.

The length of the spark gap was not measured directly, but readings were always taken, before and after a series of observations, of the spark potential of the gap in air (without admixture of petrol) at the pressure used in the series. When a petrol-air mixture is substituted for air, the spark potential decreases somewhat. A preliminary series of measurements was taken to find how the spark potential varied with the composition of the mixture ; these experiments were made with values of C and V so low that the mixture would not explode when the spark passed. It was found that the results were represented with sufficient accuracy for the purpose in view by the relation,

$$V=V_0(1-0{\cdot}6p),$$

where V_0 is the spark potential in air and V that in the mixture p at the same total pressure. Since p only varied

between 0·07 and 0·16 the total variation of spark potential due to changes in p was only some 5 per cent.

13. *Results Obtained with Petrol Mixtures.*

The complete record of the observations taken is given in Table II. At the head of each table, P is the total pressure of the gas in lb. per sq. in. (absolute); V is the spark potential of the gap in pure air at that pressure. In the following columns are given the values of p for the various mixtures

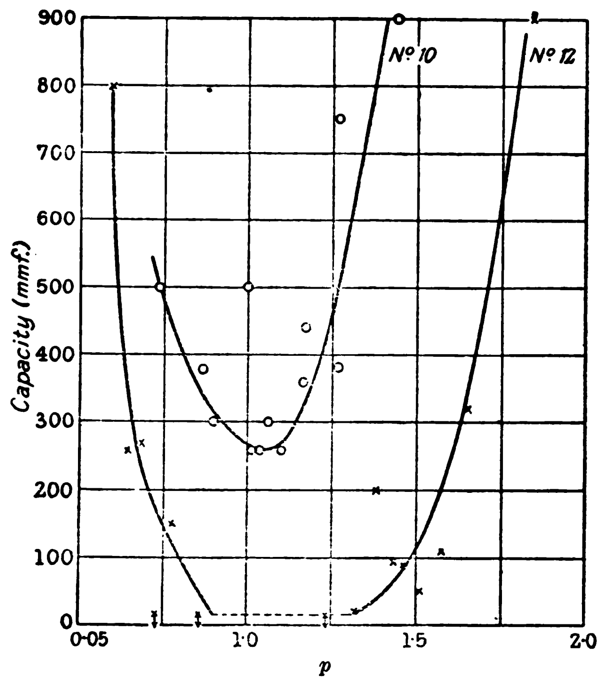

FIG. 6.—CRITICAL CAPACITY AND COMPOSITION FOR PETROL MIXTURES.

used and the corresponding values of C, the critical capacity necessary to cause explosion.

In order to indicate the nature of the results obtained and their mutual consistency, two typical series (Nos. 10 and 12 of Table II.) are shown in Fig. 6, in which the critical capacity is plotted against the value of p.* It will be seen that the

* Where the critical capacity was greater than the greatest or less than the least capacity available experimentally, the corresponding point is placed at the greatest or least capacity and distinguished by an arrow pointing up or down.

general nature of the results is clearly indicated and accords with expectation ; the critical capacity has a marked minimum and increases rapidly for values of p either greater or less than that corresponding to the most explosive mixture. But it is obvious also that the observations are not sufficiently consistent for any accurate quantitative conclusions to be drawn from them. However, since some attempt had to be made to combine the observations further, the following procedure was adopted.

A curve was sketched through the experimental points of all diagrams such as Fig. 6 in the manner shown in those

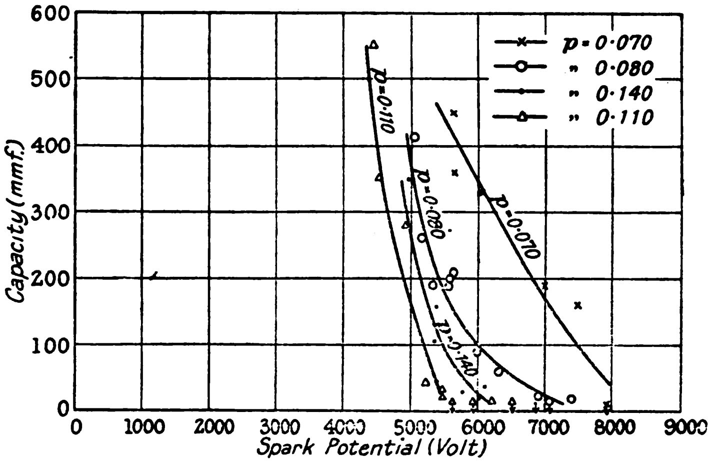

FIG. 7.—CRITICAL CAPACITY AND SPARK POTENTIAL (PETROL MIXTURES).

diagrams. From these curves values of C, the critical capacity, were taken off at various values of p. A comparison of the resulting set of values of C for various values of p, of the spark potential, and the pressure of the mixture seemed to indicate that C was a function of p and of the spark potential only, and did not depend on the pressure of the gas, except in so far as this pressure determines the spark potential; that is to say, if we keep the chemical composition of the mixture (i.e., p) constant and vary the pressure and the spark gap together in such a way that the spark potential is constant, then the value of C, the critical capacity, is constant. It is not certain, or even probable, that this proposition is generally true ; but it is not unlikely to be true over the comparatively

narrow range of spark lengths and pressures used in these observations. Since the accuracy of the observations was certainly not sufficient to prove it untrue, it was adopted as a necessary means to co-ordinating the results further.

Adopting this assumption we obtain, for several values of p, C as a function of the spark potential. The values so obtained are plotted for typical values of p in Fig. 7, in which the abscissæ are the spark potentials, the ordinates the critical capacities. The process of smoothing the curves in the

TABLE II.

1. $\left.\begin{matrix}P=91\cdot3\\V=6640\end{matrix}\right\}$

p.	C.
0·092	90
0·114	22
0·126	40
0·118	16
0·121	16
0·123	16
0·126	16
0·131	50
0·136	30
0·143	50
0·151	55
0·160	90
0·079	60
0·092	22
0·082	70
0·101	16
0·087	22
0·081	22
0·078	60

2. $\left.\begin{matrix}P=72\cdot3\\V=5840\end{matrix}\right\}$

p.	C.
0·117	30
0·136	40
0·146	90
0·165	900
0·096	60
0·084	110
0·074	320
0·051	900
0·106	55
0·128	100
0·119	45
0·129	60
0·140	100
0·154	900
0·123	70
0·110	33

3. $\left.\begin{matrix}P=81\cdot4\\V=5860\end{matrix}\right\}$

p.	C.
0·125	23
0·150	220
0·130	16
0·136	30
0·094	60
0·085	170
0 064	500
0·076	250
0·088	120
0·096	20
0·108	20
0·112	16
0·120	16
0·131	20
0·139	27
0·144	60
0·156	100
0·167	900

4. $\left.\begin{matrix}P=62\cdot7\\V=4800\end{matrix}\right\}$

p.	C.
0·110	300

5. $\left.\begin{matrix}P=53\cdot1\\V=4760\end{matrix}\right\}$

p.	C.
0·110	550

6. $\left.\begin{matrix}P=72\cdot3\\V=7900\end{matrix}\right\}$

p.	C.
0·064	130
0·124	16
0·143	30
0·152	45
0·166	90
0·186	170
0.216	900
0·052	230

7. $\left.\begin{matrix}P=62\cdot7\\V=7450\end{matrix}\right\}$

p.	C.
0·080	16
0·090	16
0·140	16

8. $\left.\begin{matrix}P=72\cdot3\\V=6300\end{matrix}\right\}$

p.	C.
0·142	16
0·166	160
0·092	16
0·080	180
0·063	900
0·072	90
0·083	90
0·095	16
0·154	90

9. $\left.\begin{array}{l}P=62\cdot7\\V=5860\end{array}\right\}$ 10. $\left.\begin{array}{l}P=53\cdot1\\V=5280\end{array}\right\}$ 11. $\left.\begin{array}{l}P=62\cdot7\\V=5620\end{array}\right\}$ 12. $\left.\begin{array}{l}P=81\cdot4\\V=7280\end{array}\right\}$

9. p.	C.	10. p.	C.	11. p.	C.	12. p.	C.
0·120	110	0·144	900	0·110	16	0·151	50
0·098	130	0·116	440			0·143	95
0·110	20	0·103	260			0·132	20
0·084	130	0·110	260			0·146	90
0·092	130	0·126	750			0·124	16
0·100	50	0·100	500			0·138	200
0·114	20	0·116	300			0·158	110
0·129	16	0·101	260			0·184	900
0·146	260	0·116	360			0·165	320
0·138	900	0·126	380			0·086	16
0·125	30	0·090	300			0·072	16
0·133	60	0·086	380			0·064	260
0·152	400	0·074	500			0·060	800
0·095	60					0·068	270
0·078	70					0·077	150
0·082	260						
0·074	260						
0·069	420						

13. $\left.\begin{array}{l}P=91\cdot3\\V=7780\end{array}\right\}$ 14. $\left.\begin{array}{l}P=53\cdot1\\V=8250\end{array}\right\}$ 15. $\left.\begin{array}{l}P=53\cdot1\\V=5580\end{array}\right\}$ 17. $\left.\begin{array}{l}P=91\cdot3\\V=5440\end{array}\right\}$

13. p.	C.	14. p.	C.	15. p.	C.	17. p.	C.
0·069	250	0·07	16	0·120	150	0·124	21
0·077	16	0·09	16	0·097	16	0·151	900
0·073	16	0·13	16	0·108	16	0·135	300
0·066	290	0·14	16	0·134	150	0·130	60
0·165	110			0·128	200	0·115	170
0·158	300			0·116	90	0·184	16
0·148	90			0·090	16	0·072	700
0·143	16			0·078	250	0·076	280
0·148	270			0·071	580	0·078	250
0·141	90			0·087	55	0·084	250
0·128	20			0·143	900	0·089	400
0·135	16			0·133	200	0·094	220
0·168	650			0·118	540	0·110	60
				0·107	30	0·103	16
				0·098	16	0·096	16
						0·118	22

preceding stage has clearly not been carried far enough, for the points are still scattered; but again they are sufficient to indicate the general nature of the relations. C decreases very rapidly as V increases, the rate of increase being greater the more easily explosive is the mixture. (Points nearer the origin indicate explosive conditions requiring for ignition smaller values of C or V). There is no indication whatever

of an approach to a critical minimum capacity common to all values of p or independent of the spark potential; there is nothing in the experiments to indicate that a capacity, however small, would not ignite the mixture if only the spark potential to which it is raised before discharge were sufficiently great.

The curves of Fig. 7 represent completely all the quantitative relations which can be deduced with any certainty from the observations. It would be absurd in view of the inaccuracy of the results to attempt to express the relation between C

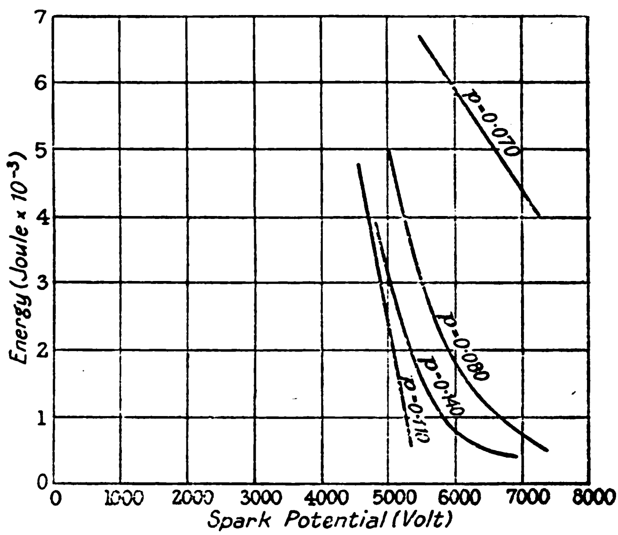

FIG. 8.—CRITICAL ENERGY AND SPARK POTENTIAL (PETROL MIXTURES).

and V as an analytical function or to base on their precise form any theoretical conclusions. But it is interesting to transform the curves slightly to bring out another point. When the quantity of electricity CV discharges across the gap, the electrical energy degraded is $\frac{1}{2}CV^2$ and this energy must also be supplied in order to cause ignition. In Fig. 8, deduced immediately from the curves in Fig. 7, this energy required for ignition is plotted against the spark potential. It will be seen again how very rapidly the energy decreases as the spark potential is increased; a change of 10 per cent. in the spark potential may involve an increase of 100 per cent. in the energy required for explosion.

One further matter may be mentioned. It was noted in §5 that Q, the quantity of electricity passing in the spark, was only found to be actually equal to CV, when C and V did not exceed a certain limit; when the limit was exceeded irregular measurements, sometimes of the wrong sign, were observed. The explanation which was offered would indicate that the limit is determined largely by the surroundings of the spark gap and that it does not represent any essential change in the nature of the spark. A series of observations was made to determine whether there was any sudden change in the igniting power of the spark in the region where the readings of Q became irregular; no indication of such a change could be found, but the inconsistency of the observations was such that small changes might easily have been overlooked.

14. *Experiments on Hydrogen-air Mixtures.*

The work of all previous investigators has shown that there is no very essential difference between the results obtained with mixtures of hydrogen and air and those with mixtures of hydro-carbons and air; the general relation between the "intensity" of the spark and the composition of the mixture is the same, the differences being merely numerical. Accordingly it was thought at this stage permissible to abandon petrol for a gas, which is so much easier to handle; and that such conclusions as were sought might be safely transferred from one fuel to the other. The remaining experiments were made with mixtures of hydrogen and air at atmospheric pressure and temperature. The mixture was contained in an explosion vessel made from a 3 inch shell through the walls of which the electrodes were supported by insulating bushes. One of the electrodes (which was always the kathode) was carried by a micrometer screw the nut of which was fixed in the bush, the junction between the two being made air-tight by rubber tubing. The screw could thus be turned from outside the vessel and the distance between the electrodes varied without interrupting the experiments. The method for controlling the discharge was unaltered.

The vessel was perfectly gas-tight and the mixture was often made by exhausting the vessel, filling to a known pressure with hydrogen and then filling up with air; but when a large number of observations were made on the same mixture a large volume of it was prepared in an external vessel. The composition of the mixture is denoted throughout

by r, which is the ratio by volume of hydrogen to the total mixture; it may be noted that the value of r corresponding to complete ignition of the oxygen in the air is 0·296. The pressure in different experiments varied between 761 mm. and 774 mm., the temperature between 17° and 21·5°C. No account is taken of these variations, but, of course, the variation in the experiments of any one series, taken at the same time, is much smaller.

15. *Stepped Ignition.*

In some preliminary experiments an attempt was made to discover the cause of the inconsistency of the measurements to which reference has been made previously. No success was attained, although much care was taken in the preparation and filling of the mixture. Moreover the inconsistency is as great when the composition of the mixture is such that it is near that of maximum explodability (so that small variations in composition should have very little effect) as when it is on the limits of explodability. It is certain, therefore, that variations in the chemical composition of the gas is not the cause of the inconsistency.

But in the course of these observations a curious fact was noted. In most of them the variation of the igniting power of the spark was effected by changing the distance between the electrodes (and so the spark potential), instead of by changing the capacity, as in the previous experiments. It was observed that, though at successive trials different values of the spark potential with a given capacity were required for ignition, these different values always tended to be grouped round two, or sometimes three, values; that is to say, sometimes a spark potential a was required and sometimes b, but never a spark potential clearly intermediate between a and b. The explanation which first suggested itself was a defect of the screw moving the electrodes, but it was eventually established, by actually measuring the spark potential in each case that the discontinuity in the spark potential required actually existed.

The experiments of Thornton were then remembered, which led him to speak of "stepped ignition." What he found was that, as the composition of the explosive mixture was varied continuously, the intensity of the spark required for ignition varied discontinuously; at certain stages a very slight variation in composition would produce a very large change in the

critical intensity, while variations in the composition between the limits at which these sudden changes occurred produced no change at all in the critical intensity. His conclusions have not been confirmed by other observers, but since here again an appearance of discontinuity (though of a rather different kind) was found, it was thought worth while to investigate the matter further.

Accordingly detailed experiments were made on the relation between the capacity and the spark potential necessary to

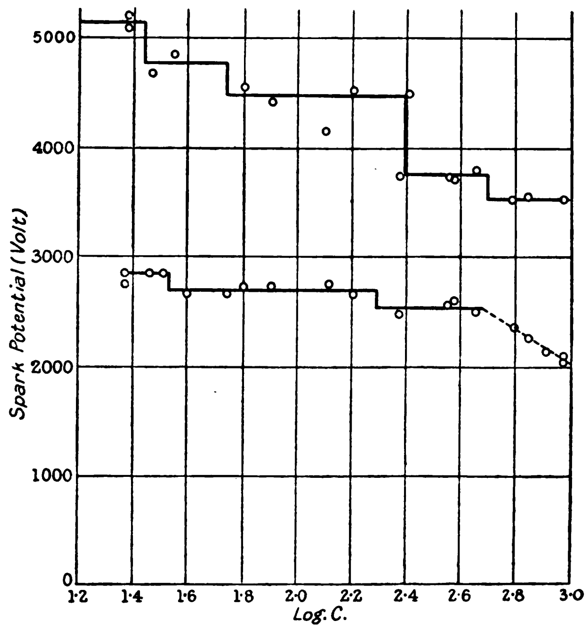

FIG. 9.

cause ignition, the mixture remaining the same throughout any one series of observations. The results thus correspond to those shown in Fig. 6; the only difference being that, while in Fig. 6 the relation was established indirectly through observations in which first the effect of varying the capacity was determined and second the effect of varying the spark potential. now the relation could be established directly because both the capacity and the spark potential could be easily varied.

The results seem to confirm the suspicion that the variation of the igniting power of the spark with the capacity or the

spark potential is discontinuous; two typical series of observations are given in Fig. 9, each series referring to a mixture of different composition. The spark potential necessary for ignition is plotted against the logarithm of the capacity in parallel with the gap. (The logarithm is chosen only to compress the scale of the diagram.) In the upper series, if not in the lower, the results are certainly represented better by a "stepped" line than by any continuous curve. The only point which lies notably off the stepped line occurs where a "step" is unusually large, and where, therefore, it may be suspected that an intermediate step has been omitted. Again, the ever present inconsistency (from which the two series of observations selected are unusually free) showed itself in the appearance of an observation on the wrong step rather than in a position between two steps.

Much more elaborate and detailed experiments than were possible in a research mainly directed to practical ends are necessary to settle the matter; but the results are distinctly confirmatory of Thornton's conclusions. Moreover, a reason why others have failed to repeat his results can be suggested.

If reference is made to Fig. 11 (which will be discussed in detail later) it will be seen that all the points lie on a smooth curve, and that there is no trace whatever of "steps" or discontinuity. Now the observations plotted there (or at least some of them) were made under precisely the same conditions as those of Fig. 9; some of the observations from one figure might be plotted on the other. The difference between the two figures is that while in Fig. 9, which shows discontinuities, the spark potential requisite for ignition is plotted against the capacity, in Fig. 11, it is plotted against the composition of the mixture. Now the general form of Fig. 6 shows that the igniting power of a spark is very much more sensitive to changes in composition than to the capacity discharging in the spark; that is to say, a given percentage change in composition involves a very much greater percentage change of critical capacity. Accordingly the effect of substituting as abscissa composition for capacity is much the same as diminishing very greatly the scale of abscissæ in Fig. 9, while the scale of ordinates is unchanged. But if the abscissæ were so compressed in Fig. 9, the discontinuities would become very much less noticeable than they are, and the curve would appear as smooth as that of Fig. 11.

Here, we believe, is the reason of the failure of Wheeler and others to observe "stepped ignition." All those who failed used the "induction" spark and varied the intensity by means of the primary current broken. Thornton, on the other hand, varied the capacity. A given percentage change in primary current changes the igniting power very much more than a given percentage change of capacity, and the failure to observe stepped ignition with the induction spark may be due simply to the difficulty of varying the primary current in sufficiently small steps.

On the other hand, the experiments, if they are reliable, show that the discontinuity does not lie in the chemical properties of the mixture, as Thornton suggested, but in the physical characteristics of the spark. Such a change of view would probably make the phenomenon easier to explain; but while its existence is still not quite certain it would be premature to suggest theories to account for it.

16. *The Influence of the Electrodes on Igniting Power.*

All the experiments hitherto described were made with the same electrodes for the spark gap, namely, steel balls $\frac{3}{16}$ in. in diameter. Preliminary experiments had shown that the capacity and spark potential required for ignition depended to some extent on the nature of the electrodes; systematic observations were now taken. In these the capacity in parallel with the gap was constant and equal to 23·8 mmf.; the igniting power of the discharge was varied by changing the distance of the electrodes and so the sparking potential. In order to multiply the observations and give generality to the results, the igniting power was always determined for a series of different mixtures.

In the first series of observations the anode was always a steel plate 1·8 cm. in diameter while the dimensions of the kathode, which was always of iron or steel, were changed progressively. Since the sparking distance never exceeded 0·13 cm, the anode may be regarded as an infinite plane. The following kathodes were used: (*a*) a sphere 1·0 cm. in diameter, (*b*) a rod 0·254 cm. in diameter with its end turned to a form approximately hemispherical, (*c*) a rod 0·098 cm. diameter, with its end similarly rounded, (*d*) a needle 0·050 cm. in diameter, with a sharp point.

The results of these observations are given in Fig. 10, curves 1, 2, 4, 6, in which the spark potential necessary to cause

ignition with the capacity of 23·8 mmf. in parallel with the gap is plotted against r which defines the composition of the mixture. (In order to avoid confusion, the experimental points are omitted from curves 2, 3, 4; they lie much nearer to the smooth curves than those for curves 1, 5, 6; the curves in Fig. 11 indicate the degree of consistency attained in respect of these curves).

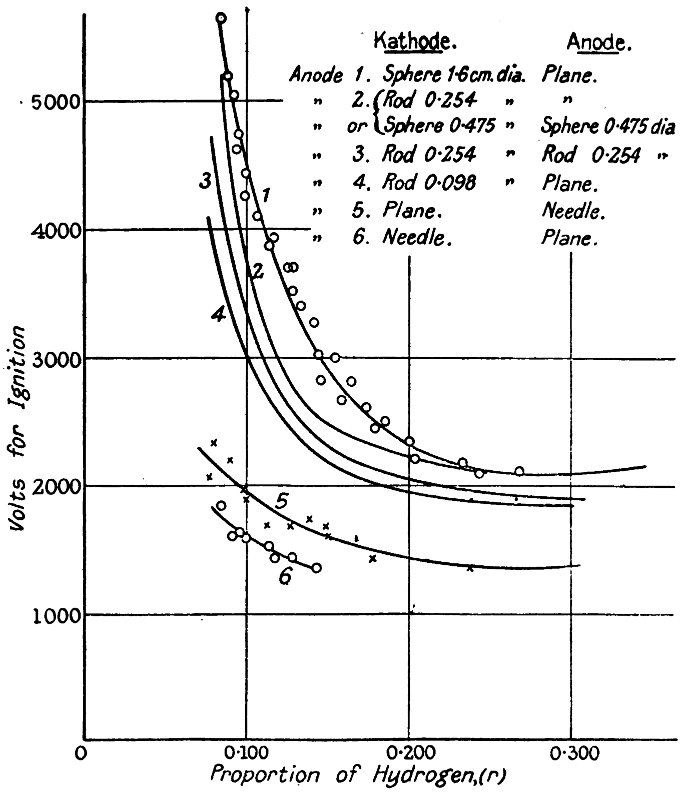

FIG. 10.

It will be seen that the spark potential required to ignite a mixture of given composition decreases notably as the radius of curvature of the sparking surface of the kathode is decreased, and that the decrease is more marked for the weaker, and less easily ignited mixtures.

Some further observations were made in which the plane surface was made the kathode and the electrodes of varying curvature was anode. In the case of the sphere of 1·6 cm. radius, no difference in igniting power due to the sign of the electrodes could be detected; such a result is to be expected,

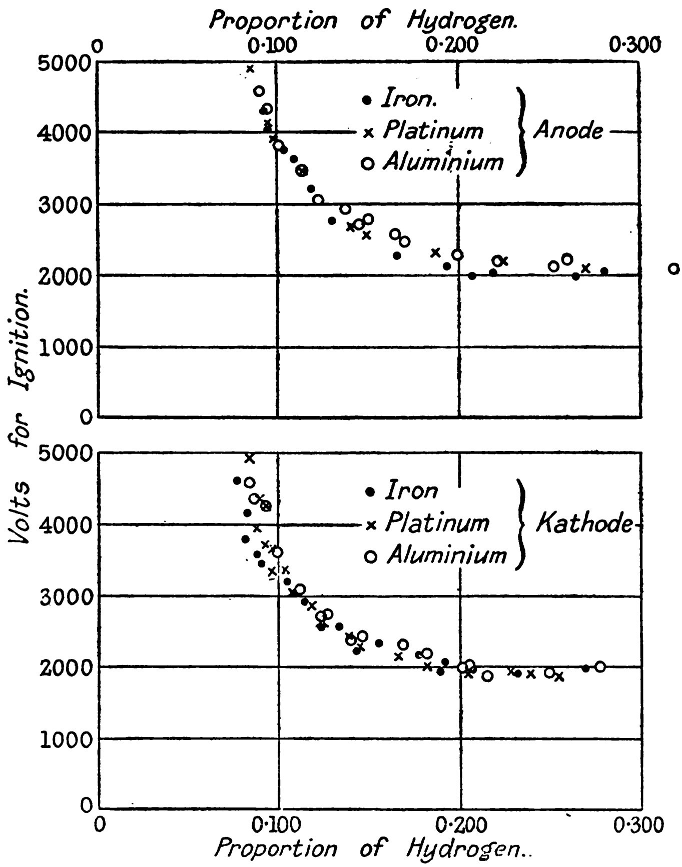

FIG. 11.

for, since the diameter of the sphere is large compared with the sparking distance, it can hardly differ materially from the plane which forms the other electrode. On the other hand, the reversal of the needle point and plane gap produces a marked change in igniting power; the gap is decidedly less efficient in producing ignition when the point is positive (curve

5) than when it is negative (curve 6). The observations when the rod 0·254 cm. in diameter was the anode are not given in Fig. 10, but the observations of Fig. 11 to be described presently, indicate that again ignition is slightly less efficient when the finer electrode (the rod) is the anode than when it is the kathode. No experiments were made with the rod 0·098 cm. in diameter as anode.

Lastly, measurements were made when the two electrodes were similar: (*c*) when they were both spheres 0·475 cm. in diameter, (*d*) when they were both rods 0·254 cm. in diameter, (*e*) when they were both needles 0·050 cm, in diameter with sharp points. The electrodes (*c*) gave results indistinguishable from those of curve 2; electrodes (*d*) gave results which are plotted in curve 3. With electrodes (*e*) the results obtained were so irregular that they have not been plotted; but it did not appear that two points were any more efficient in producing ignition than the arrangement of point and plane. It may be pointed out that the observations on (*c*) and (*d*) fit in well with those when one electrode was a plane, if we regard the average curvature of the electrodes as determining the ease of ignition, for two spheres each 0·475 cm. in diameter give the same ignition as a rod 0·254 cm. in diameter and a plane, while two rods 0·254 cm. in diameter are intermediate between a rod 0·254 cm. and a rod 0·098 cm. in diameter, the other electrode being a plane.

Some experiments were also made to determine whether the material of which the electrodes are composed has any influence on the igniting power of a spark which passes between them at a given spark potential and with a given capacity in parallel. Since it has been shown that there is such a marked effect, due to the form of the electrodes, it is necessary to secure that electrodes of different composition have precisely the same shape*; accordingly, a plane was selected as the form of the

* Thornton has announced that the igniting power of "condenser sparks" is affected by the material of the electrodes. It is difficult to suggest any explanation of the order in which he places different materials in respect of ease of ignition; and his results have not been confirmed by those who use "induction sparks." Since he does not mention the possibility that the form of the electrodes, apart from the material, may have an influence and since the electrodes he used were all made from wire, it is very probable that the differences he found were due to variations in the form of his electrodes. On the other hand, an influence of the material of the electrodes on the igniting power of the "break spark" appears to be well-established; but since that "spark" is really an arc in which the discharge is doubtless conditioned by the material of the electrodes, there is no inconsistency between this result and the experiments now described.

electrode of variable composition. The other electrode was in all cases a steel rod 0·254 cm. in diameter.

Three metals were selected for examination, steel, platinum and aluminium; they were used both as anodes and as kathodes. The results of the measurements are shown in Fig. 11, where, as before, the spark potential necessary for ignition is plotted against the composition of the mixture. The upper half of the figure refers to the plane of variable composition as anode, the lower half to that plane as kathode.

The measurements do not establish any effect due to the composition of the electrodes; but on the other hand, they are not sufficient to establish certainly that there is not some small effect of this character. In each case the sparking potentials required with an aluminium electrode are consistently somewhat greater than those required with an iron electrode, platinum agrees more nearly with iron when it is a kathode and with aluminium when it is an anode. On the other hand, the differences between the results with any two different metals are not greater than those found between different series of observations, taken on different occasions, with the same metal. Again, the differences, such as they are, are more apparent with easily exploded mixtures when the variable electrode is the kathode and more apparent with mixtures difficult to explode when that electrode is the anode. Such discrepancies suggest that the differences found are due to accidental causes.

A few less systematic observations were also made with a view to discovering an effect of the material of the electrodes. First a brass rod was substituted for the iron rod of 0·254 cm. diameter without producing any apparent change in the igniting power. Second, there was substituted for the iron disc, 0·254 cm. thick, which formed the plane electrode a disc of the same dimensions of ebonite, covered with tinfoil. The other electrode in this case was the rod 0·098 cm. diameter, or the sphere of 1·6 cm. diameter. In neither case could any difference between the iron and the tinfoil disc be established.

The experiment just mentioned, in which an ebonite disc covered with tinfoil was used, was suggested by an idea that the thermal conductivity of the electrode might be important; an electrode of this nature would have a much lower thermal conductivity than any electrode of solid metal. But a simple observation made subsequently showed that such variations of thermal conductivity as it is possible to produce practically

are unlikely to have any effect. A small fragment of wax, melting at 48°C., attached to the surface of the tinfoil electrode, was found to be unmelted at the conclusion of the observations. If the surface of an electrode of such small conductivity does not reach a temperature of 50° during a single explosion, a further reduction of this temperature by increasing the thermal conductivity of the electrode would not be likely to have much effect. No influence of the thermal conductivity would be expected until the temperature reached by the electrode momentarily came near to the temperature required for ignition by a hot body; the conduction cf heat from the electrode would have to be reduced to an extent difficult to attain in practice before this condition was fulfilled.

17. *Discussion of Effect of Electrodes.*

It is clear then that the igniting power of a spark is not determined completely by the spark potential when the capacity in parallel is given; the form of the electrodes has a very considerable influence. The results suggest at first sight that the igniting power, for gaps of different form, may be more nearly determined by the sparking distance than by the sparking potential. The relations between the sparking potential and the sparking distance for the gaps used were investigated, and are employed in the following table, together with the values of the sparking potentials deduced from Fig. 10, to give the sparking distances which are necessary to cause ignition in two different mixtures and with various forms of electrodes.

TABLE III.

—	Sparking potential (volt).	Sparking distance (mm.).	Sparking potential (volt).	Sparking distance (mm.).
	r=0·100		r=0·250	
Sphere, 1·6 cm. diameter ...	4,580	1·04	2,330	0·48
Rod, 0·254 cm. diameter ...	3,800	0·87	2,330	0·48
Rod, 0·098 c,. diameter ...	3,070	0·89	2,060	0·46
Needle point (anode)	1,980	0·64	1,455	0·33
Needle point (kathode) ...	1,640	0·69	—	—

r is the ratio by volume of hydrogen to total mixture.

The figures show that though the variation in the sparking distance necessary for ignition is certainly less than that in the sparking potential, yet it undoubtedly exists. To obtain

a complete specification of the igniting power of a gap some much more complicated function of its geometrical form must be introduced.

The physical significance of the main conclusion attained is obvious. The less the radius of curvature of the surfaces between which the spark passes, the less will be the volume of gas in which before the passage of the spark, a given electric intensity prevails, and the less therefore, in all probability, the volume actually occupied by the spark when it does pass. If the same quantity of electricity is conveyed (as it will be if the capacity in parallel with the gap is the same) and the time of passage is not very different, the current density and the intensity of ionisation will be greater in the gap with the finer electrodes at the same sparking potential. The greater efficiency of the finer electrodes at the same spark potential is an indication that the intensity of ionisation produced by the spark, that is to say, the number of ions per c.c. existing at one time, is a very important factor in determining ignition.

18. *The "Explodability" of Hydrogen and Air Mixtures.*

Incidentally to these experiments some observations were made which deserve brief mention. Measurements of the pressure in the explosion chamber after the explosion had taken place indicated the proportion of the amount of hydrogen originally present which had been burnt in the explosion. It was found that in no case was all the hydrogen burnt as the result of a single explosion; above a certain limit of r the proportion burnt was independent of r, but below that value if decreased rapidly. (All the mixtures investigated were such that the proportion of hydrogen present was less than that required to consume all the oxygen in the air; if larger proportions were used, the ratio of hydrogen consumed would, of course, again diminish). In Fig. 12 the results of a systematic series of observations are given; the proportion of the hydrogen originally present which is consumed is plotted against r, which is the ratio by volume of hydrogen to total gas in the original mixture. A large number of other observations were taken incidentally to the main measurements, and it was found that the precise proportion of the hydrogen burnt at any given value of r varied considerably from time to time; in any one series the values were consistent, but repetition at another time would give rather

different results. Thus, for the maximum proportion burnt, when r lay between 0·11 and 0·29, values ranging between 0·942 and 0·888 were obtained on different occasions. But all series agreed in showing a very rapid fall of the proportion burnt when r fell below 0·11 and in all cases the least value of r which would give a mixture which would burn at all was very near to 0·075.

The failure of ignition to be complete is doubtless due to the cooling of the flame when it reaches the walls and it is to be expected that the exact ratios observed would depend

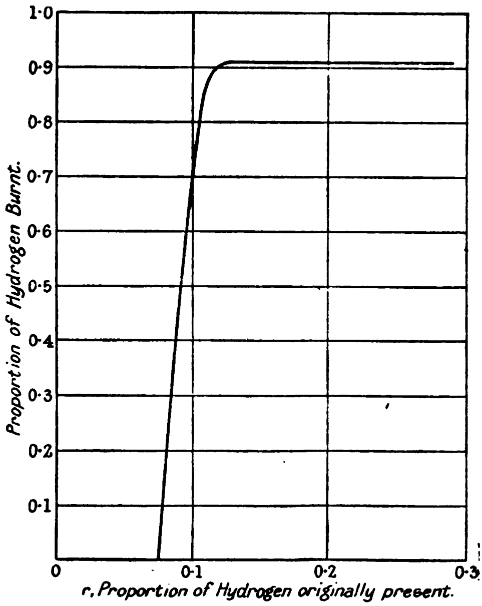

FIG. 12.

on the nature of the vessel and on the degree of turbulence. On the other hand, it is probable that the limits of r, between which a proportion of hydrogen is burnt which is greater than zero and less than the maximum for the particular vessel employed, is independent of the vessel and characteristic of the mixture only. It was shown by repeated observations that the proportion of hydrogen burnt was completely independent of the nature of the spark used to start the ignition ; whether the spark potential and the capacity were such as only just to cause ignition at all or whether they were increased

to the greatest values practicable, the result of the explosion was precisely the same. Such a result is, of course, to be expected so long as the discharge is limited to the single spark which occupies a time short compared with that taken by the propagation of the flame throughout the mixture.

Observations of a similar nature have been recorded by many others who have investigated explosion. The distinction between a mixture that will "explode" and one which can only be "ignited" has already been established; it would appear that the range in the value of r throughout which the mixture is capable of ignition, but not of true explosion is that represented by the sloping part of the curve in Fig. 12. Richer mixtures than these are capable of propagating the flame throughout their volume and so are capable of true explosion; poorer mixtures cannot even start the flame and are incapable of ignition.

19. *Practical Applications.*

Any direct application of these results to the practical problems of engine ignition must involve an assumption concerning the relation between the form of discharge employed here and that given by the magneto. What this relation is has been suggested in § 8; it was there suggested that an induction spark would have the same igniting power as a capacity spark if in the two forms of spark the same quantity of electricity was conveyed between the same electrodes. Experiments to test this conclusion had been planned. but it is unlikely now that we shall carry them out; perhaps the problem will interest others.

However, another question of great practical importance was settled by experiment. Fig. 8 shows that in suitable conditions the energy required to explode such petrol mixtures as are employed in internal combustion engines is only a few thousandths of a joule, when the spark potential is as low as 4,000 volts, and is less than one-thousandth when the spark potential is 6,000 volts. The spark potential between the terminals of a sparking plug usually lies between these limits* and hence it seems that the energy required for ignition should not be greater than that just stated. Now the energy

* The temperature of the mixture in the engine cylinder, even when starting, is usually much higher than that of the mixture in these experiments, It is to be expected, therefore, that the energy required for ignition should be even less than that stated.

dissipated in the spark given by a magneto is usually about 0·1 joule, and in that given by a battery and coil ignition set about 0·03 joule. According to these experiments, these quantities are greatly in excess of what is required; the preference shown by some engineers for the magneto as against the coil on the grounds of its greater energy would seem to be groundless, and the efforts to increase still further the energy given by the magneto spark entirely useless.

But one important assumption is involved in the acceptance of this conclusion. We have only been concerned to determine what energy is required to produce an explosion at all; it is still possible that the explosion produced by a very feeble spark might develop less power than one produced by the expenditure of greater energy. The experiments described in § 10 indicate that there is no such difference between the effects of sparks of different energy, but the matter was one which ought to be settled definitely. In order that the practical bearing of the results might be as definite as possible, the experiments made with a view to settling the matter were carried out on an actual aeroplane engine working at full power as well as at lower powers; it was investigated whether the power developed when ignition was caused by a spark with only just sufficient energy to cause ignition at all was any different from the power developed when ignition was produced by the spark given by a magneto and dissipating a much greater energy.

The engine used was an 8-cylinder 70-H.P. Wolseley-Renault engine driving a dynamo brake by means of which the output could be measured; the magneto was of the B.T.-H. A 8 type. A switch was arranged so that the sparking plugs of the cylinders could be connected either to the magneto with the usual distributor or to a special igniting arrangement. This arrangement consisted of a transformer supplying direct current through a valve to a large condenser and thence through a resistance of 15 megohms, sufficient to isolate the spark gap in the sense of § 4, to a special distributor which connected the circuit to the sparking plugs in succession at the right moment for ignition. A micro-ammeter inserted between the high resistance and the distributor enabled the average current supplied for ignition to be determined. By adjusting the filament temperature of the valve the current supplied could be regulated so that in the interval between two successive sparks just enough electricity was supplied

to charge the spark gap up to its sparking potential; this condition was known to have been obtained when any further reduction of the current produced "missing" in the engine, showing that the spark failed to pass. From a knowledge of the current supplied, the sparking potential and the number of sparks per second the capacity discharging across the gap could be estimated. It was found to lie between 80 mmf. and 112 mmf.; it could be increased by placing a condenser in parallel with the sparking plugs, but owing to the necessary capacity of the somewhat complicated leads it could be reduced no further.

The experiments need not be described in detail, for the result of them was that the special igniting arrangement proved quite as efficient as the magneto with all petrol-air mixtures, from that which gave optimum power to those which were so ill-proportioned as to cause misfiring with the magneto. On switching over from the magneto to the special arrangement no decrease whatever in the power output of the engine was observed; when the engine was working at full power a decrease of 1 per cent. would certainly have been noticed. Now the magneto, according to tests made by its makers on another instrument of the same type, gave 0·09 joule per spark. In one experiment when the crank-shaft of the engine was revolving at 1,643 r.p.m., and the spark potential was 4,500 volts, regular ignition was obtained with a current of 33 micro-amperes; since there are four sparks for each revolution of the crank-shaft the energy per spark was

$$\frac{33 \times 4{,}500 \times 60 \times 10^{-6}}{1{,}643 \times 4} = 0{\cdot}0014 \text{ joule,}$$

or about 1/65 of the energy given by the magneto. It is clear, therefore, that a very great reduction in the energy per spark given by the magneto would cause no loss of efficiency in ignition, and that any attempts to increase still further the energy of the spark are utterly mistaken.* Judged by modern standard, the compression in the engine on which these tests were made was low; the spark potential in a modern aeroplane engine with the same sparking gap would be about 6,000 volts, and the energy required for ignition would be

* It may be desirable to increase the whole energy output of a magneto in order to make it send a spark across a "leaky" plug. But such considerations are quite beyond our scope; we are considering only the energy of the spark when it passes.

still lower. It should be observed also that the figure given is not necessarily the least that is sufficient to cause proper ignition, for there was no evidence that, if the capacity in parallel with the gap could have been further decreased, ignition would have failed.

Summary.

The Paper is a continuation of that on p. 197.

(1) The previous part of the Paper showed that the "capacity" spark is the normal form of spark discharge, and that other apparently different forms of discharge differ from it only in consisting of a series of "capacity sparks," instead of only one. The present experiments were directed, therefore, to a more complete investigation of the igniting power of the capacity spark.

(2) It is shown that discharges which consist of a series of similar sparks have the same igniting power as a single spark of the same character; that is to say, that the ignition, if it occurs at all, occurs at the first spark.

(3), (4) The main experiments on the ignition of mixtures of petrol and air are described.

(5) The results show that the igniting power of a spark increases with both the capacity discharging and the spark potential, but varies much more rapidly with the latter factor. The energy required for ignition decreases rapidly as the spark potential increases, and there is no indication that, if the spark potential were sufficiently increased, the energy required for ignition might not be reduced greatly beyond the least measured in these experiments, namely about 0·0004 joule. The variation of the critical "intensity" of the spark (*i.e.*, either capacity or spark potential) is of the same nature as that found by other workers.

(6) The remaining experiments were made on hydrogen-air instead of petrol-air mixtures.

(7) The relation between the capacity and spark potential required for ignition is investigated more carefully. Indications are found of a phenomenon closely similar to the "stepped ignition" of Thornton, but the discontinuity seems to lie in the qualities of the discharge rather than in that of the mixture. It is suggested why others have failed to repeat Thornton's results.

(8) The influence of the electrodes on the igniting power is investigated. It is found that, with the same spark potential and the same capacity in parallel, the electrodes with the smaller radius of curvature give the greater igniting power. The materials of the electrode appears to have no effect on the igniting power.

(9) The results described in (8) are discussed.

(10) Some incidental measurements on the proportion of hydrogen burnt in the explosion are discussed. The distinction between mixtures which will explode and those which will only ignite appears clearly; the distinction is independent of the igniting spark.

(11) Some practical applications of the results are considered. Direct experiments on an aeroplane engine show that the energy required for satisfactory ignition is very much less than that in the spark given by an ordinary magneto or a battery and coil system.

DISCUSSION.

Dr. Eccles said that, apart from the conclusions arrived at, there was a great deal to be learnt from the Paper. The ingenious arrangements in the diagram of apparatus included, for example, a thermionic valve used as a limiting device. It seemed worth while emphasising that these thermionic vacuum tubes were going to be invaluable as variable resistances in investigations in all branches of physics. The use of these tubes assisted in the present research in eliminating the oscillatory discharge of the condenser through the spark-gap. This, in fact, appeared to be a principal difference between the operation of the present apparatus and that of a magneto and sparking plug as ordinarily used, for it is incredible that the magneto discharge takes place without oscillations; the capacity and inductance of the leads themselves will ensure the presence of oscillations of high frequency, which may have a bearing upon the time taken to convert the initial electrical energy into heat energy in the spark-gap.

Turning to the question of the ignition of gaseous mixtures, what is required in a spark, whether electrical or from flint and steel, is that a quantity of heat should be given to a small mass of matter in such a way that the temperature reached is sufficient to inflame the mixture round it. This in its turn must develop heat by combustion so fast that the next layer of mixture shall reach the temperature of ignition. If the process proves to be "catching," the wave of inflammation will spread through the whole mass catastrophically. Evidently, for the success of this process, it is necessary that the heat shall be deposited more quickly than the thermal conductivity of the surrounding matter can carry it away, and therefore a given small quantity of heat is the more effective the smaller the time and the mass in which it is developed and the lower the thermal conductivity of the medium. From this point of view it is, therefore, extremely surprising to hear that magneto makers have aimed at increasing the energy per spark. The speaker would like to ask if the authors consider their experiments, which were likewise against the joules per spark point of view, accorded better with the simple aspect of the matter just outlined.

Mr. E. H. RAYNER said that from the diagram he thought the upper condenser could be made to give an oscillatory spark if required. In the case of plug sparking, the rate of rise of potential was as important as the actual voltage reached because of leakage.

Prof. LEES asked if the authors had formed any conclusion as to the mechanism by which the ignition is produced. The importance of the potential seemed to indicate that it was the speed of the electrons that mattered.

The AUTHORS, in reply to Dr. Eccles' remarks, communicated the following: The thermionic valve does not damp out the oscillations in the condenser which discharges through the gap; it only prevents certain other condensers discharging through the gap. There was distinct evidence that the discharge was oscillatory—at any rate, when the discharging condenser and the spark potential exceeded certain limits. The single sparks mentioned in the Paper doubtless often consisted of many oscillations; but these oscillations could not be separated, partly because their frequency was so great, partly because of the difficulties to which reference was made in stopping the discharge once it has started.

In the discharge produced by a magneto there are oscillations of two kinds. First, oscillations of the spark-gap circuit only, during which the terminals of the magneto act as free ends of the circuit; the frequency of these oscillations is probably (according to experiments by Mr. Albert Campbell and Mr. Dye) about 10^8; these oscillations are all contained in the single spark investigated in sec. 8, and are of the same nature as those which would occur if a suitable charged condenser were substituted for the magneto. Second, there are oscillations of the circuit which includes the spark-gap and the secondary of the magneto; the frequency of these oscillations is usually between 1,000 and 10,000. They give rise to the successive sparks, each probably involving oscillations of the first kind, which are seen when the discharge is viewed with a revolving mirror or the rotary spark-gap.

Dr. Eccles is doubtless correct in dividing the process of ignition into two stages, in the first of which combustion is initiated, in the second of which it is propagated throughout the mixture. In the second stage the process is undoubtedly thermal. Our experiments are not sufficient to determine finally whether the first stage is also thermal; but it is our opinion that it is not. We do not think that the development of a certain quantity of heat in the mixture is essential to the starting of ignition, but rather the development of a certain intensity of ionisation. This intensity of ionisation may be produced by thermal (*i.e.*, thermionic) means; it is probably so produced when ignition is started by a hot wire, and possibly, though not probably, when it is started by a flame; but it may be produced also by other means—*e.g.*, ionisation by collision or the action of ionising rays. The first of these means is probably employed in the spark discharge; the second when the ignition is started by the incidence of X-rays on a metal plate.

We do not quite understand why Dr. Eccles says that his view (*i.e.*, that the development of a small quantity of heat is essential to the starting of ignition) is inconsistent with the view that "joules per spark" are the determining factor in ignition. It appears to us that the two views are necessarily associated.

A Demonstration of Some Acoustic Experiments in Connection with Whistles and Flutes. By DR. R. DUNSTAN.

EXPERIMENTS were made with hollow spheres, cylinders and cones with holes of various sizes and in various positions. Bernouli's theorem, which gives the wave-length of the sound produced by a cylindrical pipe in terms of the length of the pipe and an end-correction depending on the diameter only, was shown to be quite inadequate for practical purposes, the pitch depending on many other factors, such as the wind pressure, the size and shape of the blow-hole, &c. Cylindrical flutes appear to require an end-correction which—within certain limits—is equal to D^2/d, where D is the diameter of the pipe and d the mean diameter of the mouth hole (which is often oval in shape). In the shortest flute experimented with, which was only ½ in. long, Bernouli's theorem would give the wave-length as 2 inches, whereas it was actually 14 inches.

The conclusions drawn from the experiments are that in blowing across a hole in a hollow body a force existed on an elastic substance. The result is a "*spring back*," which produces an aerial throb, puff or pulsation. The frequency of the pulsation is determined by relations between the dimensions of the instrument, the size of the hole, the wind pressure, &c. Any resulting sound has its wave-length determined by the frequency and not *primarily* by the dimensions of the instrument, as in the usual text book treatment.

DISCUSSION.

Prof. BRAGG referred to Lord Rayleigh's work on open pipes and flasks and said that since the internal pressure of the air would vary with the size of the aperture it was easily conceivable that a difference of pitch would result when a change was made in the size of the aperture.

Mr. F. J. W. WHIPPLE outlined Lord Rayleigh's explanation of the long wave-length of the notes obtained from a hollow sphere.

Mr. T. SMITH asked if the variation in pitch with the position of the hole along the cylinder had been fitted to a formula—for instance, was the change in pitch proportional to the square of the distance of the hole from the centre of the cylinder ?

Mr. NICOL spoke of the end correction, and stated that students often applied the correction to the *closed* end of pipes, which was incorrect.

Mr. F. E. SMITH said the end correction was useful in electrical problems, and gave an illustration of the variation of D^2/d in an electrical case.

The PRESIDENT asked if there was much variation in the ratio D/d, and said he thought the formula was important if it could be verified over a wide range. He referred to Lord Rayleigh's experiments on flasks as closely analogous to those of Dr. Dunstan.

In reply, Dr. DUNSTAN stated that the range of D/d was not very large in the instruments he had used.

A Demonstration of a New Polariser. *By* Mr. G. Brodsky.

In the course of experiments with polarisers built of piles of glass plates disadvantages due to bulkiness of the apparatus and loss of light had to be overcome.

The idea occurred to him to place the pile of plates between two prisms of the same glass in such a manner as to—

(*a*) Reduce the length of the polariser by one-half;

(*b*) Utilise the full aperture of the pile; and

(*c*) To get rid of *all* reflected light.

Results obtained with experimental prisms he showed were so good that they could be considered a very fair substitute for Nicol prisms of corresponding size, and the very small amount of light escaping through crossed prisms (which could be reduced further by additional plates) is for most purposes negligible.

There would be no difficulty in building such polarisers to any required size, as all the material consisted entirely of glass in unlimited quantities and at reasonable price, and it was hoped that this invention (Brit. Patent 121,906) would be used for many purposes.

Polarisers for directly transmitted light were hitherto very scarce and costly, so that many uses they could be put to remained undeveloped (such as stereoscopic kinematography).

The new polariser could also be adapted to advantage in microscopes, saccharimeters, optical pyrometers, &c., and used for optical benches in the lecture room.

Experiments with piles of glass plates showed a very large discrepancy between the calculated and observed angle for best extinction. Whatever the glass used, and whatever the quality of the surface, this discrepancy came consistently to some 10 deg., whereas thin microscope cover plates were found to be useless.

There seemed to be still an interesting field for investigation as to the conditions affecting the surface of glass plates used in polarisers.

DISCUSSION.

Mr. T. Smith said it was difficult to discuss the merits of various forms of polariser, as many forms had been proposed and much was of a confidential character. He had been struck by the presence of two opposite tendencies in constructing such apparatus. In one group the optical portions of the apparatus were made large, and efforts were directed to increasing the size;

in the other group where the attainment of the same ultimate object was in view these parts were made as small as possible.

The President asked if it was not possible to get equally good results without prisms by reflection only. He thought prisms made the arrangement complicated.

Mr. Brodsky, in reply, explained that he agreed with the remarks about reflection, but that his efforts were confined to transmission effects for a particular purpose.

A Demonstration of the Uses of Invisible Light in Warfare.

By Prof. R. W. Wood, *Johns Hopkins University.*

The first device shown was a signalling lamp consisting of a 6-volt electric lamp with a small curled-up filament, at the focus of a lens of about 3 in. diameter and 12 in. focus. This give a very narrow beam, only visible in the neighbourhood of the observation post to which the signals were directed. In order to direct the beam in the proper direction, an eyepiece was provided behind the filament. The instrument was thus converted into a telescope, of which the filament served as graticule. When directed so that the image of the observation post was covered by the filament, the lamp, when lit, threw a beam in the proper direction. In many circumstances the narrowness of the beam was sufficient to ensure secrecy; but sometimes it was not desirable to show any light whatever, and filters were employed to cut out the visible spectrum. By day a deep red filter, transmitting only the extreme red rays, was placed in front of the lamp. The light was invisible to an observer, unless he was provided with a similar red screen to cut out the daylight, in which case he could see enough to read signals at 6 miles. By night a screen was used which transmitted only the ultra-violet rays. The observing telescope was provided with a fluorescent screen in its focal plane. The range with this was also about 6 miles.

For naval convoy work lamps are required which radiate in all directions. Invisible lamps for this purpose were also designed. In these the radiator was a vertical Cooper-Hewitt mercury arc, surrounded by a chimney of the ultra-violet glass. This glass only transmits one of the mercury lines—viz. $\lambda=$ 3660 Ä.U., which is quite beyond the visible spectrum. Nevertheless, the lamp is visible at close quarters, appearing of a violet colour, due to fluorescence of the retina. The lens of the eye is also fluorescent. This gives rise to an apparent haze, known as the "lavender fog," which appears to fill the whole field of view. Natural teeth also fluoresce quite brilliantly, but false teeth appear black.

Reverting to the use of the lamps at sea, they are picked up by means of a receiver consisting of a condensing lens in the focal plane of which is a barium-platino-cyanide screen the full diameter of the tube. An eyepiece is mounted on a metal

strip across the end of the tube. When the fluorescent spot has once been found somewhere on the screen, it is readily brought to the central part and observed with the eyepiece. The range is about 4 miles, and the arrangement has proved invaluable for keeping the ships of a convoy together in their proper relative positions by night.

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METROLOGY IN THE INDUSTRIES.

A meeting of the Physical Society of London was held at the Imperial College of Science, South Kensington, on Friday, March 28, 1919, Prof. C. H. Lees, President, in the Chair.

DISCUSSION ON "METROLOGY IN THE INDUSTRIES."

The President said: We have met this evening to discuss the question of "Metrology in the Industries." I think we all realise how very important the question is at a time like this, when the competition between the industries of this country and of foreign countries seems likely to become more severe than it has ever been in the past. I do not propose to occupy your time, but will call upon Sir Richard Glazebrook to open the discussion.

Sir R. T. Glazebrook, C.B., F.R.S., Director of the National Physical Laboratory: Mr. Chairman, ladies and gentlemen, I propose to speak briefly on this matter for various reasons, but mainly because I cannot in any way claim to be the originator of this discussion. Dr. P. E. Shaw, of Nottingham, who has taken great interest in metrology, suggested that there should be a meeting, and asked if I would contribute, and I said Yes, but without any intention of introducing the discussion. There are many here who can speak of the contact between metrology and the industries from more intimate knowledge than I can.

The connection between metrology and industry is, I suppose, really a very ancient one. The old Egyptians who built the pyramids must, if we are to trust what we are told by Egyptian antiquarians, have had a very considerable knowledge of metrology and of the method of applying it to building construction. So, too, must other ancient builders. Solomon, when he built the Temple, and Noah, when he built the first great ship, must clearly have used measuring apparatus of some kind, and although I do not want to go into the early history of metrology, such little knowledge as I have of it shows me that the early history of weights and measures is really a very interesting study. But I take it that in these early days, although people worked to considerable accuracy, if one may judge from what we hear and read about the Pyramids, still

the limits of accuracy were very wide. The application of metrology to industry—and by industry I rather mean the work of the mechanical engineer—to any high degree of accuracy is much more recent. I suppose we owe that application chiefly to Sir Joseph Whitworth. Sir Joseph Whitworth's surface plates and his measuring machine and gauges all were examples of the way in which the science of accurate measurement was applicable to industry, and the manner in which he succeeded in applying that science showed itself in the great accuracy and high value of the products of his firm, though, if one speaks of the accuracy of the applications of metrology, I am inclined to think that possibly the surveyors may claim to have reached even a higher pitch of accuracy than he did. If we look up the records of the piercing of the Simplon Tunnel, and consider how nearly the two lines, from the Italian and from the Swiss side, met in the centre, we shall understand that metrology as applied to engineering work of that kind and to surveying work has reached a very high position indeed.

Now, the value of Sir Joseph Whitworth's work was this, he taught people to make their delicate measurements of length with great accuracy, and he introduced—I am not sure that he was the first to introduce, but at any rate he extended its application—the use of gauges for accurate mechanical work. I think I am right in saying that the gauges he used were mostly of the nature of reference gauges. Some means of measurement were employed to compare the results of the work with the accurate gauges. The next step—and I do not know to whom that next step was due—was the introduction of the method of limit gauges. Perhaps some of the manufacturers here present could tell us rather more about its introduction. Instead of instructing a man to turn or grind a shaft 6 in. in diameter with a certain accuracy—say, of 5/1000 in.—and giving him a micrometer or some measuring machine to show him how near he gets to it, you give him two gauges, one 6 in. in width, the other, say, 5/1000 in. less, and all the workman has to do is to grind his work until the larger gauge will go over it, making sure that he has not ground it so far that the smaller gauge will go over. Thus, you have reduced the operation of gauging practically to a mechanical affair, and you do not want in that case anything like the same skill or the same knowledge on the part of the workman as you clearly do if he has no limit gauges to work to, but only some standard piece with which he compares his work by means of some method of measurement.

The first point then I want to make is this, that if you are going to produce work in quantity—if you are going to do repetition work of any kind to a large extent—you must make use of this system of limit gauging. Again, suppose you are only dealing with work produced in one and the same shop. It is not then necessary that the limit gauges should be accurate to any one standard dimension ; provided the whole system of gauges you are using is consistent within itself, your work will be interchangeable work. It is not necessary, then, so long as you are concerned with work coming from a single shop, to be very particular as to the exact unit of length to which the gauges conform. But now suppose you go further, and want to make sure that the portions of work you are producing in a large number of different shops will be interchangeable, then it is necessary that not only the gauges in each shop should be consistent amongst themselves, but that all those gauges in every shop should be referred to some definite standard; that gave rise to the necessity for accurate gauges made in large quantity and identical or practically so with each other. So far as I am concerned, my own attention was first called to this particular point—the difficulty of getting interchangeable work from various shops—by some events which took place shortly after the beginning of the Boer War. It was found that the ammunition, especially the breech screws and other parts of guns, was not interchangeable when it arrived at the front, and a committee was appointed by the War Office, as a result of which a standard lathe was made, which is now at the National Physical Laboratory, in order to assist in the production of really correct leading screws for the various shops doing Government work throughout the country. That led up to the establishment of the Engineering Standards Committee on gauges, of which Sir Frederick Donaldson became chairman, and in the formation of which Colonel Crompton and other gentlemen played a prominent part; that committee set to work to consider the problem of producing accurate gauges and of defining the limits and tolerances that were required for interchangeable work. At first we had very little knowledge indeed as to the amount of these tolerances, or as to the value of the limits suitable for work. Then our ignorance on that score was relieved by a large piece of work undertaken by Mr. Attwell at the National Physical Laboratory. He spent some considerable time measuring up work of various kinds at a large number of shops throughout

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the country, and specifying the actual tolerances and allowances that existed in practice, and from that the Committee deduced certain rules as to tolerances and allowances; so that by 1914 something had been done as to determining the tolerances allowable on gauges, and a certain number of firms had introduced the principle of limit gauge work, but, as a general rule, one may say that principle was not used generally in the shops of the country. Then came the cry in 1915 for a large supply of munitions and for the manufacture of munitions on a great scale, parts being made at various shops and at various institutions, all of which must be strictly interchangeable. The first scheme that was developed was one whereby the contractors made their own gauges, and, as any of you who have realised the difficulty of gauge making will appreciate, that led only to worse confusion. Few firms then were in a position to make gauges, and the few firms that were in that position were almost immediately swamped by the amount of work they were asked to do. Besides, there were other difficulties. It is true that drawings existed for the gauges, and firms could be told to make their gauges according to these drawings. But the real standard was not the drawing or the figures—the dimensions—indicated on the drawing; in too many cases it was a standard set of gauges in some Government department, and how near those gauges came to their nominal sizes was not known, with the result that the work first made was far from interchangeable, and difficulties arose. Sir Henry Fowler then took the matter up, he arranged with Mr. Ryan as to the supply of gauges and asked us at the National Physical Laboratory if we could do anything to facilitate the test. Just let me remind you of one step which was taken at once. By going carefully into the question of gauging of screws—by applying the simple principles of metrology to screw gauging—it was clear that you could at once halve the number of screw gauges which were needed for the work which was being put in hand. Half the screw gauges required originally were absolutely useless, and Sir Henry Fowler was able to strike them off at once. Of course, at first there were great difficulties. Gauges came in in considerable numbers, and when we began to test screw gauges the number of rejections was enormous—something like 75 to 80 per cent. At the end of two years' work, the figures went almost exactly the other way, and 75 to 80 per cent. of the gauges were passed. We had to face from time to time angry committees, who were

out to "win the war" with the least possible delay, and who found it difficult to appreciate that a few thousandths of an inch in a gauge made a difference. But many of those who were at first inclined to think the accuracy required and much that was being done quite unnecessary remained to bless after all. The reason for this was that through the war this interchangeable work proved itself absolutely necessary.

Now, I want to make as my second point, the point that if we are to maintain our position in peace, interchangeable work of this kind in engineering manufacture is equally absolutely necessary. If that is the case, parts of machines must be made to be interchangeable, so that we may have manufacture in quantity. Sub-division of labour is essential. Much is due to those who have produced this result during war-time, the persons who at the Laboratory and elsewhere studied the various problems and devised the methods of testing and the machines for testing, but far more, I think, to those who throughout the country threw themselves so heartily into the bringing to real practical effect the lessons that were taught by applying metrology to this great industry.

I need not go into the special difficulties of the measurements; I think they are well known. Much has yet to be done if we are to keep ahead. And if I may spend a few moments further on that point, I would say that in the first place I think it absolutely necessary that we at the Laboratory should improve our own appliances and our own methods of working to the very utmost. Then we must secure, I feel sure, that research should accompany all the testing work and examination work which is done in connection with gauges, as, indeed, with everything else. Improved methods of investigation grow out of the examination of the work that is being done; and important work follows from those methods of examination. It is necessary that testing work and research work in this as in other matters should go hand in hand. Any scheme which contemplates a separation between these two branches of work is to my mind, doomed to failure. But while that is so, and while I think it is necessary that there should be one authority looked to in the country to set up and maintain the standards, it is desirable also that there should be at various places throughout the country local institutions and organisations for testing and issuing tested gauges. This, I think, is desirable from many points of view. It is one way of interesting the manufacturers locally, and making them

acquainted with the advantages and with the necessity of accurate gauge work, and it is also a means of educating the persons who are going into industry, and who will help to carry on the work of manufacture by limit gauges through the instruction and teaching that they have received in such institutions. All such institutions must be closely linked up together, and every effort should be taken to secure that there is no setting up of various standards for various parts. We shall hear more on this subject from Dr. Shaw. We shall hear from him an interesting account of what he has been able to do with regard to education in Nottingham.

I trust I have indicated briefly some of the reasons which lead me to think that the applications and teaching of metrology to engineering manufacture are of the very greatest importance, that while we have done in the past few years a considerable amount of work there is yet much to be done, and for that work we need the co-operation of the manufacturers in the country and of those who realise that it is only by research of a somewhat advanced character that the progress we are looking for can be maintained.

Mr. Wm. Taylor (of Messrs. Taylor, Taylor & Hobson, of Leicester) said: We have all listened with great interest to the excellent sketch which Sir Richard Glazebrook has given us of the progress of metrology during the war. I have no doubt that as a result of the war this country and other countries will realise great advancement in this subject.

We are too apt to think, however, that all this progress has been made during the war, and to overlook a great deal of work which was done before the war. I believe that interchangeability in manufacture has existed in the watch industry, especially in Switzerland, for a century or even centuries. In the scattered farmhouses of Switzerland it has been the custom to make the parts of watches interchangeably to gauges—the jewels, the pivots and so on—and the parts have been brought down into the valleys for assembling. And in watchmaking the tolerances are very small—of the order of 1/10000 in. One reason why our own country has been behind some others—for it has been behind—in interchangeable manufacture is because we have not had here any flourishing manufacture of clocks and watches, which is a training ground for highly skilled mechanics accustomed to precise work. In America interchangeable manufacture, as we understand it to-day, together with the use of limit gauges, was first introduced, we are told, by Colonel Colt during the Civil War. From Colonel Colt's works it spread through concerns now well-known in the Eastern States, and long before it was generally understood in this country, interchangeable manufacture was being fairly widely practised in America. Nevertheless, it was practised in this country. I believe I am right in saying that when the watchmaking industry of Lancashire was a flouri hing industry, the parts were made as in Switzerland, and were interchange-

able. Many other manufacturers in this country have been carrying on interchangeable manufacture. The manufactures of sewing machines, bicycles and of ball-bearings are well-known examples, and, if I may mention my own firm's work, it is about thirty years since we first established the manufacture of the turned and threaded parts of photographic lens mountings within a tolerance of 0·001 in. interchangeably, and since I first made for measuring the Whitworth screw thread trigonometrically, the notched bar and the needles and prisms which have been so useful during this war.

Some beautiful work has been done by our makers of printing types, the widths of the faces of which have been brought within something like a tolerance of 1/10000 in. They have used microscopes, but have become so expert that it was said of one great maker of printing types, Sir Henry Stephenson, that with his unaided eye he could look at a type face, and say whether it was 1/10000 in. too large or too small.

We must not blame our manufacturers for not having been so advanced as we think they should have been. One disability was this: The Americans have the advantage of being specialists in manufacture, and they have a large market of their own, which is protected; we, on the other hand, are not to the same extent specialists, we have a relatively small market, and not even the unrestricted use of that. It is exceedingly difficult to restart the clock and watch industries in this country, because we are up against the competition of huge businesses established elsewhere, in one alone of which they are making from three to five thousand watches in a day. That is one reason why we have been backward in this country; we have not had large markets, and we have not specialised sufficiently. Perhaps, through greater specialisation and the economy which will accompany specialisation our manufacturers will find their markets in spite of handicaps.

I noticed in reading the advance proof of Mr. Shaw's Paper, which will be read presently, that he made mention of the need of teaching in our schools. I do sincerely hope that our engineering schools will begin to get busy in a new direction. Far too long they have been busy teaching the science of design and neglecting the science of the workshop. We have to dig out the science which should govern our workshop practice. It has not been brought together and put in form for teaching. I hope, for example, that somebody will write a treatise on the principles of gauging for interchangeable manufacture, and that succeeding speakers in this discussion will, among other things, tell us something more about the requirements of accurate gauging. It is important that our manufacturers should have measuring apparatus of the highest order. They must have good standards of length. They need to know whether these standards should be line measures, which are not subject to wear, but need to be translated into end measures by means of microscopes, or whether they should be end measures. If they are to be end measures, then we probably ought to develop in this country the manufacture of standards of the Swedish type, so that these may be available to everybody at very low prices. Nothing would do more to further the advancement of accurate measurement among engineers than the provision of cheap and accurate standards of this kind. We hear that Mr. Sears is developing methods of making them, and hope that he will show how they can be supplied at the cost of a few pence each.

Sir HENRY FOWLER, Ministry of Munitions, and Chief Mechanical Engineer, Midland Railway, said: The last speaker said that he came here with one object. If I have an object it is that I would like to utter an appreciation of the work done by those who have paid attention to metrology, and more especially to the wonderful work of the National Physical Laboratory, thanks to the guidance of Sir Richard Glazebrook. I am perfectly certain that the country does not know or appreciate the debt it owes to him. I was fortunate in having control of what I suppose has been the largest production job that the country has undertaken. We turned out somewhere about 200 million shells, and with that was associated the cartridge case, the fuse, friction tube, &c. The operations on a certain fuse were something like 600. It was a production job—a repetition job—of some size. Before I joined the Ministry of Munitions in 1915 I had had some experience of the difficulties which arose, not only with gauges, but in the measurement of gauges. We were, as I said, very fortunate indeed in having the National Physcial Laboratory to depend on, and I say "depend on" advisedly, because, outside those of the N.P.L., what appliances there were on which any great reliance could be placed were not, at all events, generally known. The inefficiency of the appliances which existed in some places, and which could not be relied on for giving consistent results, was the cause of very considerable trouble, and one which tended to discredit metrology. Now, in the early days I heard a good many things said against the National Physical Laboratory, but there was one thing I never heard. I never heard it suggested that any gauge which went to the National Physical Laboratory twice ever had two different reports made on it, and that is the whole matter with regard to the final opinion which came to be held as to the excellent work of the National Physical Laboratory. People grumbled at its very accuracy, but they appreciated very soon that its measurements were absolutely reliable, and they found that if the Laboratory decided that a gauge was out in a particular direction, that was the direction in which they had to work in order to get it right. Mr. Ryan, of whose work as Director of Gauges, one cannot speak too highly, will speak later and more authoritatively than I can do, and will tell you, perhaps, the difficulties which had to be overcome with regard to the screw gauges, and the way in which they were dealt with. There is one lesson which should not be lost. The lesson of the Boer War was not taken to heart, and when the great war broke out you could almost count on your fingers the number of perfect lead screws which could be used without any adjustment for the production of screw gauges. There is one thing which was always a source of worry to us, more particularly with regard to gauges, and that was a taper screw thread called the "G.S." (general service) plug. It does not taper in any well defined way, whilst it has not an even number of threads per inch. The only way in which we could think that thread was made was that it was cut when both the lathe screw and the bed were badly worn, and that the product was made the standard. That was one of the things we had to deal with. We had to try and get gauges which should meet these extraordinary conditions. We have been satisfied with regard to the screw gauges for ammunition, and although, perhaps, I am dealing more with design than with metrology, the necessity of standardisation of the various screws was

fairly well impressed on everybody. Mr. Ryan was getting perfectly happy on ordinary munitions of war until the aeronautical engine question came along, and then you had every kind of pitch and thread that had ever been thought of. The fact that, practically speaking, no question was ever raised with regard to the lack of interchangeability is, I think, the very highest testimonial which can be paid to the work of Sir Richard Glazebrook and Mr. Ryan. Mr. Taylor has said that interchangeability was in practice in this country before the war, and one respect in which this was so was the Renold chain. The fact remains that, undoubtedly with regard to the country as a whole, it did not appreciate the advantage of limit gauges. Now that we have gone to all this trouble to get interchangeability one does not want the work to be lost. It is not only a question of measurement, but of keeping the measurements right. I remember somewhere about fifteen years ago going to a motor-car works in Berlin—and the motor-car is another instance in which limits have been used for a considerable time—where the works were all sectioned up with expanded metal and a gauging system controlled each section. I was intensely pleased to see such care taken. I had a deal with certain cars made in this factory over in this country, and the very first time we bought spares we found they would not fit. It all goes to show that we may do things as carefully as possible, but we have got to bring this question down to our everyday concerns, and there is a very great deal of education still wanted in this direction. The country has had a wonderful lesson in experience, but it has not graduated yet. And there will be a tendency to go back again to the old ways unless we go on hammering at the subject. We can only produce economically if we can produce in quantity, and only produce in quantity if we go in for interchangeability, and we can only have interchangeability if this science of metrology goes ahead.

The Need of Metrology in the Universities. *By* P. E. SHAW, *B.A., D.Sc., University College, Nottingham.*

COMMUNICATED BEFORE THE MEETING.

Of all experimental sciences physics is *the* one which deals with measurements, for in no other science is there such variety, scope and delicacy of measurement performed. Metrology, the science of measuring, may be taken to embrace fine measurement of all physical factors for which a unit is definable. Its scope, however, as at present practised at the National Physical Laboratory, includes length, mass, time and simple derivatives of these, such as area, volume, angle, velocity, pressure. It may be desirable, however, before long to bring thermal, optical, electrical and other factors within the purview of the science. This subject is an applied science, independent alike of physics, of which it may be considered a derivative, and of engineering, for the purposes of which it is at present principally used.

Metrology, as we now know it, is almost synonymous with the length measurement of solid bodies, so greatly does the work performed in the metrological laboratory in this section exceed that done in all other sections put together. This state of affairs is due to the efforts of engineers, who from about 1840, when Sir J. Whitworth began his

pioneer work, have developed accurate methods of measuring over-all, under-all and other lengths in their machine parts.

Metrology made its first appearance as a separate organised science in this country about 1903, when testing work was commenced at the National Physical Laboratory. This Institution has ever pursued a generous policy of helpfulness to all comers, and under its fostering influences this new science grew rapidly in significance till 1914, when, with the stimulus of war, it at once developed into great importance. Standardisation was adopted by the nation as the only possible basis on which war munitions could be produced in the quantity and of the accuracy necessary for victory. Thus, as the essence of standardisation is accurate measurement, metrology at once took its place as an important component of the war machine: a science of great practical importance.

Although this subject has come to the engineering craft of this country as a by-product of the war, it is now permanently established, essential alike for the uses of industry and pure science.

The engineering trade is likely to extend greatly the principle of standardisation of output, as well as to adopt the standardisation schemes of the Engineering Standards Committee. For these and other purposes this trade will feel the necessity for more and more metrology.

In 1916 I undertook metrological work for a great shell factory. This led to the starting in University College, Nottingham, of a department of metrology. This was, I believe, a new idea, which might be adopted with advantage in universities and colleges—at least in those situated in manufacturing centres. The present time of transition seems opportune for a discussion of the subject. The work done in a metrology department in a university would in some respects be unique in character. It may be comprehended under three headings—Teaching, Testing and Research.

Teaching.—In organising a course of instruction in this subject one would make it chiefly, but not wholly, practical, and would only attempt it, as regards day students, in the cases of those taking final work in engineering and physics. Engineers should pass from the college to the works with a knowledge of the best modern practice as to the use of precision measuring tools. At present probably not one engineering student in a dozen on leaving college understands the rudiments of the principle of limit gauges or can claim to have had a systematic course in, for instance, the use of a measuring machine or of screw-measuring devices.

The physics student also would acquire some acquaintance of this subject, greatly to his advantage. It would add something useful to his technical equipment to know a really good measuring tool when he sees it; to know something of its manufacture, how it should be used, and of what order of accuracy it is susceptible. At present we put before him, in the physics laboratory, apparatus often necessarily rough, and his results rarely attain an accuracy of 1 in 1,000. In laboratory teaching one has too often to warn the student that his present object is to acquire knowledge of physical principles and experimental methods, and that his numerical results will often be poor. The establishment of a department devoted to accuracy of measurement would inevitably

raise the standard of the apparatus in the teaching laboratory. We cannot fail to damp in some degree the enthusiasm of the inexperienced student by having inefficient instruments for his use.

There is one industry—instrument making—in which physicists are specially interested. This craft would certainly benefit if a knowledge of metrology were more widely disseminated. We want to have more instruments made in this country. In the past we have often had to send abroad for good and reasonably cheap apparatus of all kinds; and later, when repairs are required, we not infrequently send the apparatus back to the makers, because we lack their special facilities. There will soon be a great demand for apparatus to meet the coming activity in teaching and research, and in the industrial developments of physics and other sciences. We shall need more skilled instrument makers, and definite instruction in metrology would certainly be conducive to this end.

Testing.—The metrology department would have standards, such as those of length, mass and time, verified at the National Physical Laboratory. From these as a foundation the various measuring instruments in the university could be calibrated to a known order of accuracy. In like manner we could check instruments as they arrive, whether from our own workshop or from the instrument makers; and we could forestall trouble from defective apparatus whether destined for laboratory use or for research. It is bad business in buying measuring tackle to accept it at its face value. In many cases, given suitable and properly installed methods in a metrological laboratory, it would be easy to check the accuracy—*i.e.*, the money value—of the instruments on arrival.

If this testing system were organised we could give students excellent practice in the technique of instrument testing; but organisation and system are necessary. At present, in the absence of a metrology laboratory, the busy demonstrator can spare little time from routine work and his own research for the necessary testing of laboratory apparatus. The result is that the checking of values, if ever done, is sporadic, and on these rare occasions it is done laboriously and inefficiently; for good testing work requires technical skill attained after careful study of methods and preparation of appliances.

To take one or two instances. A resistance box may go wrong. Its polished top looks imposing, engraved white on black; but after years of use, and abuse, it loses accuracy; it may even be partly burnt out. And then the engraving should be of the nature of an epitaph: "Below this slab lie the charred remains of a once useful instrument."

Again, I once had a Whitworth measuring machine lent me as a favour. It was in use, and was considered to be in order; but, on finding the action very hard, I took out the micrometer screw, and actually found parts of the thread missing and other parts rusty. So far had a well-made tool fallen by ill-usage and neglect.

Of course, the metrology department would check apparatus belonging to various other college departments—lengths for the engineers, masses for the chemists, and so on. For the physics department much could be done in tests on length, mass, time and other units.

In a provincial city this department might be of great service to engineers in testing their standards, micrometers, check-bars, screws

and gauges generally. Standardisation is likely now to be greatly developed by engineering firms. This system breaks down and becomes more or less a failure unless precision is practised throughout in tool room, workshop and inspection room.

Industries, other than engineering, might avail themselves of these facilities. Thus, I have repeatedly since 1916 performed tests on needles used by the Nottingham embroidery industry. The manufacturers found that the British made needles were inferior in action to those formerly imported from Germany. On close examination I detected small differences in dimensions between the two makes; but these differences proved to be vital, for when, on repeated trial by the makers, they were eliminated the British product worked perfectly.

There are other well-known troubles in the local textile industries, notably in the non-standardisation of hosiery needles and in the uncertain length of yarn run off for a given weight. These are problems for which metrology can supply solutions.

It may be thought by some that the National Physical Laboratory is the proper place for all this testing, and that in undertaking it we should be usurping the functions of that Institution; but I would offer one or two remarks in opposition to this view: (1) The laboratory at Teddington will have enough to do with primary work and the more difficult tests, without handling the smaller work from every industrial centre in the country. (2) It is good for manufacturers to have a testing laboratory in their neighbourhood. Without such local facilities they will tend to get along, as heretofore, without metrology. (3) A metrology department can only keep in touch with industrial needs by taking in testing work; this vitalises it. (4) So far from robbing the National Physical Laboratory of testing work, provincial departments may well act as feeders to this national testing house; the more science you give the industries the more they will want De-centralisation appears to be the best way of spreading scientific methods throughout the country. The N.P.L. will always do the highest class of work and will remain the court of appeal.

I am not unaware of the difficulties attending metrological testing. The responsible head of the department must have a special training in the subject; and he would do well, whenever possible, to institute a system of cross-testing and checking, seeing that errors in metrological measuring and calculating are easily made and are often fatal.

Research.—This would take the form in the case of metrology of improving the existing methods of measuring and of introducing new methods as required. Thus, the methods of measuring screws, both external and internal, at the National Physical Laboratory, and the general screw-measuring machine which we have produced in Nottingham (*see* "Engineering," January 24, 1919), all in the last three years, show the kind of research that is wanted. These methods are adaptable, accurate and sufficiently speedy. Their value to the engineering industry should be incalculable. It will often happen that an industry in a particular district will present a problem peculiar to that industry for solution. This could be worked out by the local metrologists in close touch with the manufacturers.

I have tried to show in this brief sketch that the general introduction of metrology in the universities would materially assist both the teaching

and the research in physical sciences carried on within their walls, and would be valuable to the industries in the surrounding district. Pure science has been in a state of suspended animation for the last four years, while scientists in general have given themselves up to scientific war work; but, while research in pure science will now be restarted, it seems certain that industry will require, and will obtain, much more assistance from men of science than in pre-war times. The old attitude of aloofness between science and industry in this country is, one hopes, now to be finally abandoned.

Dr. P. E. SHAW said he was glad to hear Sir Richard Glazebrook express the view as to the desirability of disseminating metrology throughout the country. His own scheme as outlined in the Paper already in circulation on "The Need of Metrology in the Universities" might appear to have only an oblique bearing on the subject under discussion, but, in reality, he deemed it fundamental, as suggesting a practical way of bringing metrology and industry into close association.

The most striking result of the recent great development of metrology for war purposes was the hold this science has now acquired in the whole engineering industry. It is no longer merely the cult of a staff of experts in a Government bureau working in connection with a few specialist firms, but is now a widespread science, and has permeated to every workshop, large and small, in the country. Its recent achievements are no mere flash in the pan due to the war. These are presumably destined to be greatly exceeded as to scope and depth, both in engineering and in other industries.

In setting it up in a university, teaching would be undertaken to both day and evening students. Unfortunately for the teacher, there is at present no text-book, good or bad, on the subject, though there are some important monographs on individual points. The industrial class is virgin soil for the teacher. When one is assured by works managers that a large proportion of skilled mechanics do not know how to use even a micrometer, it is evident that our artisan class needs instruction in precision measuring. Besides artisans the advanced day students would benefit by an acquaintance with metrological method and appliances. The enthusiasm of the student so often damped by using rough laboratory equipment would be kindled afresh by handling the excellent metrological apparatus now obtainable. When the inexperienced undergraduate is using the crude appliances too often found in a teaching laboratory he may well be excused for doubting whether his feet are being set in the path of an *exact* science. Besides the teaching in a university department there would have to be some testing work undertaken. Without this kind of work the subject lacks vitality. Mere academic measuring would never have the interest appertaining to tests on actual products about to be used industrially. The provincial department would derive its standards from the N.P.L., and it would have to refer to this central bureau in much of the more difficult work for which the specialised department at Teddington is so eminently fitted. As an instance of useful testing work which could best be performed by a provincial testing house, Dr. Shaw quoted work on which he had been engaged on for years in connection with the textile industries, chiefly on needles. These and the various yarns proved to be

uncertain in dimensions and strength. The obvious remedy for all these vagaries was standardisation through the agency of metrology. As another sphere of activity the university departments would prove useful centres for research on the subject. The speaker had recently succeeded in producing a screw-measuring machine, which, when in a commercial form, he expected to provide a full solution of the old problem of measuring the diameters of internal screws.

Metrology has now attained the dignity of an independent subject. It was not a pure science, like physics, or an applied constructional science, like engineering. It should stand on its own feet, and be allowed full scope for development.

Mr. F. J. Dykes: I think I can claim to combine to a certain extent the academic and commercial side of gauges, and from the latter point of view I should like to say a few words. I have had the privilege of being connected during the past year with a firm which has been making gauges for sale for the last fifteen years or so; the result of this experience is that I have come to believe that the first thing we want if we are to extend the use of gauges throughout the country is vigorous educational propaganda. In spite of munition work, we still have manufacturers who look upon gauges as cylindrical bodies made to standard sizes; such gauges are rarely useful, and the one thing we must insist on, the one gospel we must preach, is the use of limit gauges; these are the things which are wanted urgently. I am very glad to see that the Engineering Standards Committee is at last reconsidering its old decision to recommend the shaft basis for the system of limit gauges, and I think I need only say that the fact that the shaft basis system of the E.S.C. never took on, whereas the hole basis system is the only type which has reached any general use in this country, shows that the general trend of engineering opinion is in the direction of the hole basis from the commercial point of view of production. In connection with the Newall system, with which I was concerned, one great defect is that it does not go far enough. Even the A and B classes which engineers know, are too fine for many purposes, and we want one with a coarser tolerance, say, a C class, or even a D class of tolerance, which will take in every grade of work, even that allowing "sloppy" fits, such as agricultural machinery or perambulator wheels, or anything you like. We want to educate the country to understand that limit systems can be used for any production whatsoever, and that has been the hardest job in the early pioneering work. Whatever England did in the past—I am very sorry we were backward in this country—the one bright spot in the early work was the fact that Japan welcomed the limit gauge system with eagerness, and I think that points a moral. We know that the class of labour one has in Japan is largely unskilled, and if we are going to extend production here it will be to the unskilled labour we shall have to turn; we must never go back to the old system of one so-called skilled man, one job, one bit of steel. We must make it an operation production, and not a one-man job. Then, turning to the actual production of gauges, we want to make them cheaper, and one thing that can be done there is to educate the manufacturers so that they will not ask for special stuff. After the armistice inquiries came from all quarters, and, taking the screw gauges only, there were something like

two standard screw gauges wanted to 400 special threads. To give a concrete instance, motor-cycle manufacturers in very rare cases use standard threads, and we received inquiries for gauges for such hybrids as screws 18 mm. in diameter and 16 threads per inch, where ⅝ in. B.S.F. is very close indeed. Why need they go outside the list of standard screws? Besides that, they could get their gauges from stock, and every engineer knows what it means when something can be sent for from stock. Now, if I may appeal to our friends at the National Physical Laboratory, it would be to ask them to settle two classes of tolerances now for standard commercial gauges, and to announce that they are prepared to test gauges to those tolerances. People send inquiries for gauges, and ask at the same time, "Will they pass the N.P.L. test?" We were in difficulty as to answering that question, as we did not know what "passing the N.P.L." meant; also, if we could have two classes of tolerances, we should have the very good gauges we have at present, and another class, not quite so accurate, perhaps, which could be used, nevertheless, in cases where the buying of a very expensive outfit would not be justifiable; also, provided the upper limits of these tolerances were the same as in the A class, the gauge could be relegated to the B class when worn, and we should double its life—a very important point when we come to consider the outlay on gauges. That would also get over another difficulty, I think, for possibly some of our present munition tolerances have been too close. I say this because I know what excellent work had been done before the war by gauges which would certainly not have passed the N.P.L. munition test, and I do not think I am giving away any trade secrets, but am merely stating a matter of common knowledge, when I say that a number of the fuses turned out during the war would not pass all the inspection gauges, so that it was necessary to resort to selective assembly in some cases; still, the fact remains that, thanks to Sir Henry Fowler, they got the fuses out, and they certainly did their job. But if we are going to get the widespread adoption of the gauge system, I feel that we must cease to strive after ideal gauges, and have commercial gauges which will give commercial results. The ordinary manufacturer is shy of spending too much capital on his gauge equipment; when he is educated it will be different. Lastly, I should like to touch on something which arises out of metrology, and to put in a word for the optical projector, for which we are indebted to Sir Richard Glazebrook's department, as an ordinary tool-room tool. In my own experience I find that when this was first put in the men were shy of it, and refused to use it; gradually they began to use it, and now they refuse to work without it. So I hope that some manufacturer will give us a projector rather more robustly made than the present type, and one which the ordinary British workman can handle without the necessity of overhauling it once a week; I want to see the projector used as a tool, and not as a scientific instrument.

Mr. M. F. Ryan, Director of Munition Gauges, Ministry of Munitions: My excuse for taking part in this discussion is due to the fact that for the past four years I have been very intimately connected with the application of metrology in the very difficult work of establishing a supply of gauges for dealing with the inspection of munitions. When this work

was taken in hand there were two great difficulties that hampered the supply of gauges for the innumerable types of munitions, such as mines, guns, aircraft engines, and every kind of store connected with the war. These two difficulties were due to the lack of experience of accurate methods of measurement, both of those who laid down the specifications for the gauges, and also of those who were making the gauges. With regard to the specifications, the difficulty, I think, arose from the following cause: A Government department responsible for the inspection of any stores was responsible also for the gauge designs. From the gauge drawings certain standard gauges were made, and then measured up in their laboratory and established as reference standards. Our experience was that the measuring instruments used for measuring those standards were by no means close enough to measure up within the tolerances. The result was that the standards established were false, and the drawings sent out to manufacturers to work to might be accurately worked to, but the product—the gauge produced—did not fit the reference standard. This hampered the production of gauges more than anything else at the beginning of the work, because it discouraged the manufacturer, and those who drew up the specifications had a false idea of the degree of accuracy that could be readily attained to. As soon as the National Physical Laboratory took up the work, this state of affairs was quickly exposed, and the result has been that the tolerances laid down on gauges now, as compared with the tolerances at the beginning of the war, are about three times wider. We want to have as wide a tolerance as possible on the gauge, so that the gauges can be manufactured commercially. (I may mention that the production of inspection gauges just before the armistice amounted to something over 10,000 a week.) The next point is lack of knowledge of metrology in the workshops. The education of the workshops was carried out by the National Physical Laboratory (Metrology Department), and I see that Mr. Sears and Mr. Dudding are here, and they will be able to give exact information as to how they get over these particular difficulties; it is due almost entirely to their work in teaching the various gauge-making contractors throughout the country how to measure and how to measure accurately that the rate of supply which I mentioned was secured. When gauge-makers were under the control, as it were, of a Government department, and under the supervision of the National Physical Laboratory, rapid strides were made, and the progress in metrology in gauge-making works and in the tool works of the manufacturers of aeroplane engines and other parts during the last two years is probably equal to ten or twenty years' progress under normal conditions. This is due, as I remarked, to the very excellent work done and the very great care taken to help the manufacturer by the N.P.L. It would be a very good thing to extend the idea, and I am heartily in favour of what Dr. Shaw recommends—namely, the setting up of metrology departments in the various universities. Each university can do the same for its own area as the N.P.L. has been doing for the whole country. The result will be more accurate work, the people in the works will be educated, and they will not be afraid of micrometers; micrometers, indeed, will probably be obsolete in fine work, they will have to work to minimeters. But in order really to make progress it will be essential that work shall be done to limit gauges. There are

signs that many manufacturers who during the war had been accustomed to work to limit gauges are going back to their old methods, and it is only by education and by educating in particular the young school of engineers that the old uneconomical methods of produotion can be finally abolished.

Mr. J. E. SEARS, JUN., Supt. of the Metrology Department, National Physical Laboratory : I want to make a few remarks from the point of view of the National Physical Laboratory, and particularly of my department, as to our work in the past, slightly, and more particularly as to our future programme. The war has given us a unique opportunity in connection with this matter of gauges. Our intimate association with the manufacturers of gauges and also, to a less extent, but still to a considerable extent, with the manufacturers of stores for which the gauges were intended, and also with Mr. Ryan's department, which has been the life and encouragement of the work on both sides, has enabled us to attain progress quite exceptional in the history of the subject, for ourselves, as well as for those whom we have been endeavouring to aid outside. It is now quite evident, I think, that this particular phase of munition work—limit-gauge work—is going to be a very important factor in all future engineering industry, and as Sir Richard Glazebrook said at the beginning, it is evident also that limit gauges, if they are to be applied as a national proposition as distinct from a local proposition—and by "local" I mean in the individual firm, and by "national" I mean securing that the products of one firm or one industry may be interchangeable with those of another—it is evident that all the controls of such gauges must be referred to a central body governing the fundamental standards of length. That body should be the National Physical Laboratory, and the first work of the National Physical Laboratory is the maintenance of the first order standards and the very highest class of work—investigation and research—in connection with them. Metrology, of course, covers a very much wider range than merely gauges, and we hope that our sphere of usefulness in connection with the industries will not be confined solely to the particular industry of mechanical engineering, but that we shall be of service in a similar way to other industries. I might mention the sort of subjects involved. Metrology is the science of pure measurement, and it has been limited hitherto to measurements of mass, length and time, and to simple derivatives of these, such as pressure, volume and mechanical velocity or density, and we have not had any *affaire* with such other subjects—except in so far as they are incidental to our work—as optics, thermometry, or electricity. Dr. Shaw's Paper suggests that these may be part of the sphere of metrology, but the scope of the subject is already sufficiently wide.

The point I want to make particularly is that we still need to keep in very close contact, as we have been able to do during the war, with the industries for which we are working. Unless that is done, any scientific department is apt to run off the lines a little, and to chase very interesting investigations which may, nevertheless, not belong to the particular phases of work which would be of most value ; and we do want to maintain that intimate contact we are enjoying at the present moment. Metrology is rather a peculiar thing, because, although it is

a s ience in itself, as has been pointed out, it is unlike other sciences in certain respects. Metrology is not a science of great discoveries or inventions; it is the science of doing with very great accuracy very simple things, and for that reason the main stimulus to progress is to be found in the interest which one gets out of seeing its application daily to routine work of industry. In the same way we want to keep routine work closely in touch with the developments of metrological science, in order to maintain the daily work of the industries up to the best standards which can at any given stage be attained. It is also necessary for the Laboratory to do a certain amount of routine work itself in this connection, partly because that is one excellent means of keeping the association between ourselves and the manufacturers, and partly because it is only by having the experience so gained that we can really get the stimulus to advancement which we require. I do not wish in saying that to contradict Dr. Shaw's arguments at all; I think there is plenty of scope for the local centres as well as for the National Physical Laboratory, but it is important to get quite a considerable flow of test work through the Laboratory, as well as our scientfic work, in order to balance the various aspects of the case, and I hope that we shall get a good deal of routine testing work to do still, partly of a commercial kind, and partly, perhaps, official work for Government departments. It is necessary, in order to carry out that sort of work, to have a sufficient flow of material so as to organise it on a substantial basis. If you only have a few gauges coming to you for test, the trouble of getting out your standard gauges, tidying up your machines, and preparing to do the work, and then putting it all away again immediately afterwards, renders the whole operation inefficient. In order to attain efficiency you must have a reasonable demand for the class of work involved. The stimulus which the demands of the war gave to improvement of methods and greater speed and accuracy in testing has been invaluable, and we still want to have the same stimulus, although we do not want always to have the question of speed forced upon us to an extent which almost overwhelms every other consideration.

Then there is another point which I want to bring out. Accuracy of manufacture depends on accuracy of measurement, but in its turn accuracy of measurement depends on the accuracy of the measuring machines. We were, to start with, very much hampered by lack of manufacturing experience at the Laboratory. Eventually we got a quite small workshop, and that has done yeoman service in the development of the machines mentioned. The Ministry authorised the erection of a much larger workshop, which is now approaching completion. Accuracy of measurement frequently depends as much upon perfection in the manufacture of the apparatus as upon the design; and the workshop associated with the Metrology Department ought to be such as to be capable of developing manufacture on lines of extremely refined accuracy—accuracy which is not required in ordinary commercial workshops. Such a workshop should be able, as progress is made, always to do something just a little better than is required in the best current commercial practice, and to devise improved methods where these are needed. I can refer, as an instance, to Johansson gauge work. We have depended very largely during the war on the use of Johansson gauges, and there came a time when it seemed doubtful whether the

supply would be maintained. We cast about to see whether something could not be done to make them, and I am very glad to say that in our little workshop my colleague, Mr. Brookes, succeeded in producing these things, and not only so, but we have reason to belive that in a larger workshop we could produce them much more cheaply than Johansson, and also much more accurately.

That is only one instance of the sort of thing which the metrological workshops might aim at. With it, and with the aid of the efficient designing department, with routine test work as a guide and stimulus, and with the upkeep of the fundamental standards and research connected therewith as the basis of the work, the department expects to be occupied in a balanced way, and to have information available which it is anticipated will enable us to give very valuable assistance and advice to engineers or to other industries requiring such assistance; and we hope to maintain our contact with them, and to encourage them to come to us for such advice as we can give.

It does not seem to me altogether desirable that the university or technical school should take charge of test work. The necessity for teaching metrology work in the university seems to me paramount, but the conditions of a university or technical school are hardly the best fitted for the routine test work, although I quite agree that local testing centres are desirable. The skill of the operator in measuring is largely a matter of experience, and you cannot ask students who can only devote a comparatively small portion of their training at the most to this sort of work to do such testing, nor, I think, is it quite suitable work for the teaching staff. The conclusion that I come to is that for the routine work you do want a special staff to be entirely devoted to it, and I think that the local testing office would be invaluable not only to the local industries, but also to the local university as an object lesson to students of the things that were being taught; but I doubt whether actual routine testing is quite the function of a university.

There is only one other point, referring to Mr. Dyke's remarks, the Laboratory *is* considering the question of tolerances, and hopes to do something in the matter quite soon.

Metrology at the University of Sheffield. By Prof. W. RIPPER, *Ph.D.Eng. of the Department of Applied Science, University of Sheffield.*

COMMUNICATED BEFORE THE MEETING.

There is no doubt that industry has just emerged from war work with a new and additional experience provided by the production of repetition work for war purposes. Hitherto many of the older industries considered that the production of work in quantities necessarily implied a corresponding loss of quality. As a result of executing contracts for munitions, they have now found out by practical experience that it is possible to produce work in huge quantities, and by means of an efficient inspection to maintain accuracy and quality, which they previously could not obtain in the whole of their output.

The reason why the industries have been able to accomplish this when engaged on war products is because the Government has insisted upon an efficient system of inspection, which has had the effect of compelling the individual firms to adopt more scientific methods in the production of their goods.

One of the many lessons which has been learned is that of the practical application of metrology to industry, especially in the production of war material, and it is only by such application that it is possible to maintain an efficient standard of quality and at the same time obtain enormously increased quantitive output.

It is well known that the Universities and Technical Institutions throughout the country have greatly assisted the country by the use of their laboratories, equipment and staff in all departments of war work.

At the University of Sheffield the application of metrology to industry has been applied in metallurgy, engineering and glass. It would be as well to remind ourselves that Sheffield has for a very long time been doing this work for the steel industry. If we take as an example the work done in connection with engineering, the University has supplied 6,553 gauges, the accuracy of which has been of the highest grade. Latterly this work has developed, and we have been engaged in the production of hardened steel reference checks for the testing of inspection screw gauges.

With regard to the practical application of the equipment of the University which has been used for gauge purposes, we are desirous of

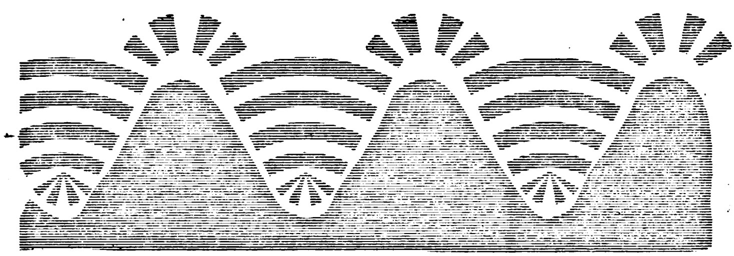

FIG. 1.—A TYPICAL BRITISH TAP PURCHASED FROM A RETAIL STORE.

assisting the local industry engaged in the engineers' small tool trade. Fortunately, this industry is already combined in an enthusiastic Association of all the firms engaged therein, and it is with this Association that our experience will prove extremely useful.

In the University a complete set of appliances for the checking of screw gauges has been installed. This is no longer required entirely for war work, and it might be very well employed as a local reference department for the checking of gauges, chasing tools, &c.

Figs. 1 and 2, which illustrate the form of thread of two taps compared with the standard form, show the improvement in the work which is possible.

The question of the best system of gauging taps and dies is, of course, still under discussion; but in the meantime the set of gauges illustrated in Fig. 3 has been made as a trial, in the hope that it may help in the evolution of a system.

Fig. 1 shows a commercial tap which has been purchased locally, and illustrates very clearly the need of applying an efficient inspection system to the work. The defects in the form of thread are so obvious

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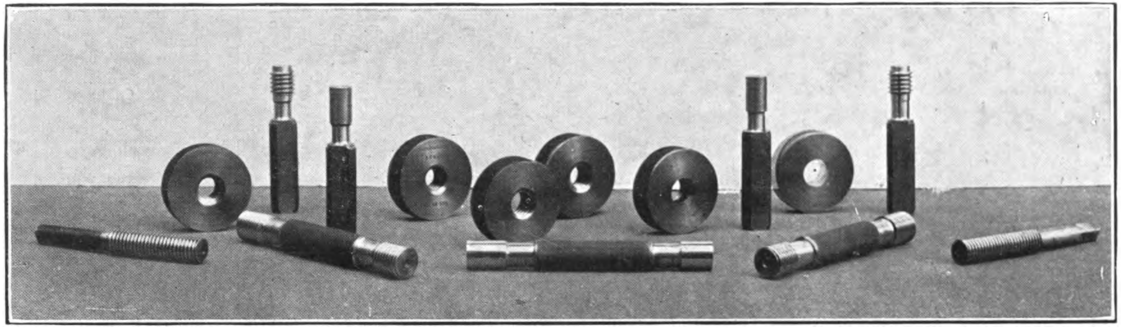

FIG. 3.—A SET OF GAUGES AND CHECKS FOR INSPECTION OF TAP DIAMETERS.

that no comment is necessary. On the other hand one can buy taps from other firms, as an example in Fig. 2, where the form of thread is commercially good, and where a satisfactory inspection system is no doubt in operation. It would be as well to remember that in pre-war days the ordinary manufacturer of taps and dies did not concern himself at all with effective and core diameters, nor was he seriously troubled with errors in pitch and angle of thread ; but with the demand for producing tools that will cut satisfactory threads to pass inspection it is necessary that all these factors should be taken into consideration, and an efficient system of inspection should be used in order to ensure that all taps and dies when manufactured should be within certain agreed limits.

This modern tendency for more accurate work is now being catered for by many firms who are able to apply metrological inspection to their work, at the same time supplying the work at a competitive price.

Fig. 3 shows a set of gauges which can be used for checking both limits of each of the major effective and minor diameters of a tap, together with a set of checks for testing the accuracy of the gauges themselves. At the extreme ends of the illustration two taps are shown.

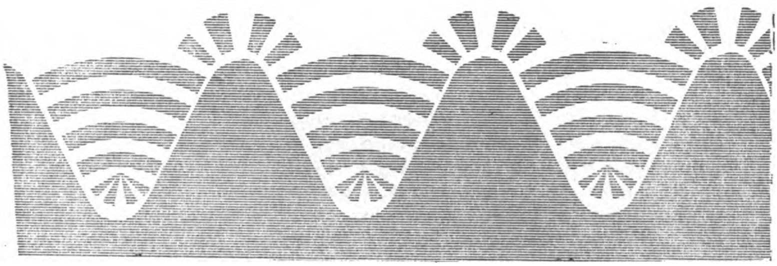

FIG. 2.—A SELECTED ACCURATE TAP.

This set was made to demonstrate to our local association a practical method by which all the diameters of the tap could be checked between the limits.

The illustration of this set of taps is not given as a standard set, but only as a set which was made to demonstrate to the Association one way in which all the diameters of a tap could be properly measured.

It does not necessarily follow that the application of an efficient inspection system in a works will retard output or increase the price. because the evidence which has been obtained during the war by numerous firms who have used inspection methods for the first time has been entirely in favour of increased production and lower prices.

The older industries of Sheffield have been awakened by a long spell of repetition work which the Government have required for war purposes, and they have seen the great advantage which has been obtained by standardisation.

The University inaugurated a course of lectures immediately the armistice was signed for the Cutlery, Edge Tool and File Trades. These lectures are given by a member of the engineering staff who had experience in the gauge work, and the lectures took the form of an engi-

neer's view of how to obtain efficient production in these various industries by an application of science to these industries.

In addition to the lectures, Technical Societies have been formed for the following: Cutlery, File, Edge Tool, Machine Knife and Saw trades, and most encouraging results have followed.

The societies are representative of manufacturers, staff and workmen, and they have all taken a very keen interest in the scientific side of their work. There is evidence of a demand for the practical application of science to their industries, and we are hopeful of research associations being formed in the near future in order to assist in the development of their industries along the most efficient lines.

What has been done in these older industries of Sheffield can be done in many other industries in the country, and one of the best methods is, no doubt, by the reading and discussion of Papers.

Mr. W. H. Bolton, Department of Applied Science, University of Sheffield: In considering this subject of metrology in industry I would ask you to look at it from two points of view—first, from the metrology point of view, how it can be applied to industry, and what are the practical steps to be taken; and, secondly, from the industry point of view, in which case we must look at industry as it exists to-day, and that not from the engineering side only, for we have had far too much engineering to-night and far too little of the general industry of the country. What steps could be taken to educate the industries so as to get them to appreciate the value of metrology? I would just ask your indulgence in considering some of the older industries. If, as one speaker has aptly pointed out, education is necessary amongst manufacturers—I presume he was referring to engineering manufacturers—in order that they might be got to appreciate metrology, what is the position when you come to compare some of the older industries where the work is still done by old rule-of-thumb methods? I would like to give you a comparison. The education of a student leaving a technical college might represent the engineering industry as compared with a child in the infants' department of an ordinary Council school representing the older industries. Not only is education necessary in these particular industries from the workman's point of view, but also from the principal's. Responsible men who are in the position of leading manufacturers do not understand the elements of metrology. They do not understand that efficiency in their business depends upon the successful application of metrology. During the war many lessons have been learned, and it has been one of the advantages of the war that it has led manufacturers to standardise and concentrate. I might mention that in the cutlery industry in Sheffield we had one firm manufacturing as many as 1,700 different patterns of pocket-knives. Now, gentlemen, we in Sheffield pride ourselves on our reputation for the production of cutlery. We claim that we produce still the finest cutlery in the world, but we have almost lost what is known as the "quantity trade," and this is due to the fact that we do not realise the value of the application of metrology to the industries. The cutlery works in Sheffield have never had educational opportunities, they have never known what the value of metrology is, and it has been to our competitors in other countries to take this advantage.

With regard to our own work in this direction, we have had some experience in the production of gauges, and we have got together a plant for inspection and for the production of this accurate work, which we do not intend to scrap. We think that the decentralisation of the means for making these accurate measurements is absolutely vital to the interests of the country. We think that the industries want vitalising by having many centres in which testing and checking can be carried out. We agree in believing that all these centres should be in close co-operation, and that their standards should be uniform, but I disagree with the last speaker in the application of testing; if this is necessary to the National Physical Laboratory in order to keep it in close touch with industry, it is more than necessary for the local Universities and Technical institutions. If we in Sheffield could put students on to testing work we should feel very proud, but if you are going to educate the best type of engineers in your technical institutions, then I submit you must have practical men to use the instruments and explain the instruments, and unless these men have some practical work to do, how are you going to get your demonstrations ? We have had far too much of the sort of teaching in our Technical institutions in which the results of the work are thrown under the bench. What better demonstration could you have for students than that of practical testing ? That applies generally to the education of engineering students. I should like to refer to one branch of engineering which we have in Sheffield—namely, the Engineers' Small Tools Manufacturers' Association. They have been working in the past, along with many other industries, on old-fashioned lines, and still to-day, I am sorry to say, they do not recognise the necessity for the successful use of gauges. They have not even been educated up to the point of being able to make their own gauges. But we have had a round-table discussion with this association, the result of which is that they have considered the problem, and have decided to work in co-operation with our local Department of Applied Science, and I believe the time will come when they will be successful, not only in adapting their methods of small tool production to metrological work, but also in producing for themselves the gauges they will need, making them to the fine limits which will be required.

With regard to the older industries, it is there, I think, where a good deal of help is necessary. Sheffield is not the only city where there are old-fashioned industries which require help. Such industries are scattered all over the country. But we recognise that the time has now come when these industries must be wakened up, when they must be brought up to date, and when they must apply metrology in their own particular crafts. We have tackled this subject by calling together the manufacturers, workmen and staff, and putting before them the advisability of a course of lectures from the point of view of engineering —that is to say, what would an engineer do with their factories and methods ? What would he do if he were in their industry ? Many engineers have considered the problem, and they have all repeated the same thing. They have said that it wants ending, that it cannot be mended. But we have formed in these industries technical societies for the discussion of difficulties, and although these societies have only been formed as recently as January they have done good work ; already one society has had five Papers, the fifth is down for to-night, and is being

given by a man who is practically self-educated and in a very small way. We have the promise of a Paper by a trade union official, who left school at the age of eleven, and altogether I am pleased to say that the workmen have taken a vital interest in this question, so that we hope by bringing the problems home to them and getting their difficulties from them to be able to educate them. But all this is theoretical work. However good the lectures may be, it is practical work which will move these men, and therefore we have taken our classes to the works. On Tuesday next we are taking a party of over 70 students—we call them students, although they are manufacturers, staff and workmen—to Manchester to see the organisation of one works there. By practical demonstration these people will be so educated as to see the possibilities of metrology. With regard to research, I believe that here is the key to the situation. We hope to be able to move the department of Scientific and Industrial Research to do something for Sheffield, and if that is possible to the extent that we require, then I believe that the Technical schools and Universities could follow on the same lines, and so bring home to the industries the necessity and value of metrology and the object lessons in its application. We believe in the engineering trade that efficiency depends entirely upon the applications of the scientific method, but I am sorry to say in many other industries they look upon this as theory. Therefore, we want education along these lines, and especially by discussions such as we have been having this evening, shall we arrive at that position when metrology will be of some practical use to industry.

Contribution by BERNARD P. DUDDING (Research Laboratories of the General Electric Co.): I think that the discussion has been too much confined to limit gauges—in fact, to a particular type of limit gauge, the screw gauge—and has not considered the large field covered by the science of metrology and its bearing on industry.

One might gather from the remarks of some previous speakers that all that is required to put English engineering in the forefront and keep it there is an unending supply of accurate gauges, and the general adoption of the limit gauge system of working.

Speaking in a general sense, before the war we were behind the principal American and Continental firms in the production of high-class machine tools, of measuring appliances and of scientific apparatus. At the present moment, in spite of some marked advances, the same criticism can be made, and as an instance, I wish to emphasise the impossibility of getting really good English-made tools to equip an up-to-date tool room.

The development of the production in England of accurate screw gauges to which considerable reference has been made to-night, has depended entirely on Pratt & Whitney measuring machines and on Brown & Sharp's micrometers—both imported articles, for which no really good English substitutes existed in 1916, and I doubt whether they do at the present moment.

I contend that what is wanted to advance the status of mechanical work in England is a thorough understanding of methods of making accurate measurements, and their application to all kinds of manufacture.

The limit gauge cannot replace this knowledge, and may only lead to the production of a large amount of second-rate articles, and with a tendency to improve the accuracy of the article.

Anyone who has had experience in branches of engineering requiring high accuracy cannot fail to have been impressed by the need for the knowledge of sound metrology in production of work of high precision.

In rougher types of work the errors can be seen without having recourse to measurement, and the corresponding improvements can be made, but as the workmanship improves the necessity for accurate measurement arises.

A manufacturer, say, of a machine tool, wishing to improve the accuracy of the machine, can only achieve this with certainty by being able to measure the existing errors or the errors of its products. Further, the experience he gains in making these measurements will be of tremendous value to him when considering new designs.

In workshops generally metrology is not used or understood, in fact, a man often uses a tool produced by a firm of reputation, and assumes it to be without error, or assumes it to produce work without error.

This is largely due to the fact that the subject of metrology has had practically no attention in technical schools and colleges, and very little standard literature can be found on the subject. It gives me great pleasure to note what has already been done to improve the education of English students in the science of metrology, by Prof. Shaw, at Nottingham, and by the Department of Applied Science, Sheffield.

I think that in most cases where metrology is introduced to help commercial engineering—*e.g.*, to introduce improvements in gear cutting, in machine tools, in scientific instruments, &c., a research will be necessary—firstly, to determine the methods of measurements to be used and possibly to develop new measuring devices; secondly, to attempt improvements in the actual production. The manner in which metrology can help the production of accurate mechanical work is well illustrated by the work that has been achieved in this country in connection with the manufacture of accurate screw gauges.

When the demand for large quantities of these gauges arose in 1915, the errors existing in the best make of English gauges were many times larger than the tolerance called for, and the average gauge made in the tool rooms of English firms had errors 20 to 30 times these tolerances. Although experience led to the tolerance being doubled, a lot of improvement was obviously needed to get quantities of gauges of the required accuracy. The errors arose principally from ignorance of errors in the tool shape and its setting and ignorance of the errors introduced by the latter.

The methods of measuring in use at the National Physical Laboratory in 1915 were not suited for application to the workshop, nor were they suited to deal with testing of large quantities of varying sizes. The staff of the Metrology Division at the National Physical Laboratory had first to investigate methods of measuring, having the above two objects in view. Machines were devised to give speed and accuracy in measuring the diameters, using needles and V-pieces, and a projection apparatus was devised to enable an image (50 times magnified) of the axial section of the screw gauge to be viewed and its shape contrasted against the standard thread form.

The diameter measuring machines were made by Taylor, Taylor & Hobson, and introduced to the gauge makers and their use explained.

Projection apparatuses were also erected in the various shops, and the workmen instructed how to use them.

The National Physical Laboratory staff also spent a considerable amount of time in the screw gauge maker's shops investigating sources of error.

The number of screw gauges tested at the National Physical Laboratory rose from 300 per week in 1915 to over 3,000 per week in late 1917. The percentage of gauges up to the standard asked for in 1917 was about 10 per cent. in 1915, but between 75 and 80 per cent. in 1917.

The English made screw gauges produced during the latter period were as good as any that could be obtained from the best makers on the Continent or in America ; in fact, with the exception of gauges made by Johansson, of Sweden, the best English screw gauges were better than those made elsewhere.

The production per person employed had also risen immensely, in spite of the heavy dilution of the skilled labour with unskilled. The unskilled workers were largely women.

In developing researches of any sort, difficulties will generally arise in getting the work from the laboratory into the shop. The first thing necessary is that the people doing the research shall have intimate contact with the man who is actually doing the work, and I would emphasise that it is very necessary to stimulate the interest of the workman.

In conclusion, I wish to draw attention to the method of projection we developed. It will lend itself to the improvement of all manner of work, and it should be widely used by precision workers. This is evidently being done in America, and the American technical press has recently had descriptions of apparatus which are only variations on the original machine which was made in England, and sent over when America first entered the war.

It would be a pity if an apparatus originated in England were to be used more widely abroad than at home.

Sir Richard Glazebrook, in replying on the discussion, said : I do not think that at this hour I have really anything to add. The discussion has been to me extremely interesting, and I am glad to find how much is being done all over the country in this direction. Mr. Bolton's address from Sheffield struck me as being particularly important, and it is very reassuring to realise that not only in the engineering industry, but in other forms of industry, the importance of metrology is being grasped, and something is being done to make what metrology can do for industry more widely and more generally known.

Letters were received from Sir Frank Heath, of the Scientific and Industrial Research Department ; and from Major P. A. MacMahon, Warden of Standards, expressing their regret at being prevented from attending the discussion.

Contributed after the Meeting by Mr. Sidney A. Horstmann, *of Bath.*

Metrology as applied to the measurements of screws is perhaps the most interesting branch of the science. The quantity production of

high precision screw gauges has shown the possibility of manufacturing screwed parts to a much finer degree of accuracy than has been possible heretofore.

Before the war little was known of the methods of measuring screws, and when the Ministry of Munitions called for quantities of screw gauges the only source of information available was the National Physical Laboratory, and it speaks volumes for this Institution that screws of an accuracy not dreamt of before the war can now be produced commercially by several firms and institutions in this country.

Measurements of length and diameter to a high degree of accuracy do not present much difficulty to the manufacturer with modern equipment, providing he has fundamental length bars or blocks for reference, but the measurement of the elements of the screw is an operation requiring far more knowledge, and the fact that a screw may measure correctly on all elements and yet be malformed still further complicates the subject.

From a gauge making point of view a screw does not present so many difficulties as it did, owing to assistance given to manufacturers by the National Physical Laboratory; but manufacturers of screw components are constantly in difficulties with their product, mainly due to the malformation of threads and pitch errors. The manufacturer may use gauges, often he does, but even if gauges are used and they fail to pass the work, the reason may not be obvious, and he has to resort to "trial and error" as a means of cure.

This all seems to point to the necessity of each manufacturer being provided with some form of projector, and sets of correct screw form, so that not only his gauges but his product as well may be reflected on the screen to a known magnification. The use of the projector is undoubtedly one of the most important advances made during the war in the examination of screws and other gauges, and its universal use by manufacturers would be of inestimable value to the industry generally. All manufacturers will not go to the expense of sets of screw gauges, especially where the article is not of standard size, and special gauges are required for checking, but with the use of a projector, preferably one that is able to check pitch errors as well, the operators are able to see the exact errors in their production, and it will be found that great interest is taken in the tool-room in producing forms of thread that will bear magnification, so that cutters, chasers, dies, &c., would all improve in form by having to be examined in this way.

Samples of work produced with die-heads, automatic and otherwise, can be readily examined, and providing the forms and pitch are reasonably right, outside diameters are quite sufficient for checking purposes. A good deal of education would be required in the various tool-rooms to help operators to correct for pitch errors in die heads, and should the product show considerable pitch error it would be necessary to examine the die-head chasers for pitch, and if these did not reveal the trouble the helix angle of the chaser would have to be checked, as this is a frequent source of trouble in die heads. This again could be done by means of a sine bar attached to the projector to which the chaser would be clamped, and which would be adjusted until a clear definition was produced on the screen of all flanks.

Measuring machines for diameter and pitch should be among the equipment of every modern tool-room, and this is generally acknowledged among manufacturers. The projector is looked upon as a luxury, but the variety of uses it can be put to makes it far more valuable than either of the foregoing, and its use would largely eliminate the multiplicity of screw gauges required especially where small quantities of articles with non-standard screwed portions are concerned.

The use of wax, graphite, sulphur and dental plaster for taking impressions makes it quite possible to examine internal threads quickly and easily.

Prof. J. B. HENDERSON, *of the Royal Naval College, Greenwich, contributed the following remarks, dated March* 29, 1919.

I regret that I had to leave the meeting last night, due to another engagement, before I was called upon to give my promised contribution to the discussion on the subject of metrology.

One very important point which was not mentioned while I was present is that of the influence of the accuracy of workmanship upon the factor of safety required when dealing with materials subject to repeated stress. No high degree of accuracy is required when dealing with ductile materials such as mild steel subjected to more or less constant stress, because the ductility of the material overcomes any defect in the fitting by local yielding. When, however, the material is subjected to alternating stress, such stress becomes a source of weakness. In order to illustrate this point I may mention the case of some test specimens which were sent by a Government department to the Engineering Laboratory at R.N. College, Greenwich, to be tested on the Haigh alternating stress machine, which gives an alternating pull and push test at a frequency of about 30 cycles per second. The specimens for this machine have screwed ends ½ in. diameter, screwed with 20 threads per inch and the parallel middle portion of the specimen is ¼ in. diameter. All engineers will agree that the factor of safety for the screw threads in these specimens is ample, and no case of failure of a specimen breaking in the thread had been experienced until these particular specimens were received of a special bronze, of which three out of seven broke in the screw thread. Whether the failures were due to badly-fitted threads or due to some peculiar property in the bronze has not yet been elucidated.

With respect to the proposed educational propaganda, a first step, and a very important one, is the education of all engineering draughtsmen, and the man in the street in general upon the significance of figures. Now that it has become general practice to dimension drawings in decimals, it is quite common experience to find a dimension of, say, 1/16 in. entered in a drawing as 0·0625 in., although tolerance in workmanship required may even amount to 1/32 in. on that particular part. The figures 0·0625 ought to indicate that the tolerance is one or two significant figures in the last place of decimals.

Educational propaganda ought to begin at the very bottom in the elementary schools by teaching the children in simple multiplication to multiply by the most important figure first; to deal with the thousands before dealing with the units; contracted multiplication and division would then become quite natural.

The President, in closing the discussion, said: I have no special knowledge of the subject under discussion, but, as a layman, it seems to me that one or two points have emerged from the discussion with very great prominence. The first is, I think, that under the stress of the last five years' work, many manufacturers have learned to make use of limit gauges, and by use of them have turned out work which they would have said was quite impossible five years ago. Another point is that there is a tendency at present for them to fall back into the old ways, but if we allow that tendency to go on we lose something for which we have fought vigorously, and which we ought, if possible, to retain. It is in this connection that the suggestion of Mr. Dykes seems to me worthy of very careful consideration, namely, that for those manufacturers who hold that in their business the highest type of gauge with its small tolerance is not necessary, there ought to be provided an inferior class of gauge with a wider tolerance. In the course of time these manufacturers might find by experience that even the highest type of gauge would be useful to them. Another point that seems to have been brought out by the discussion is the advisability of having distributed over the country sub-stations, at the universities, or elsewhere, where the accurate measurement of gauges could be carried out. These stations would be run either by the National Physical Laboratory, or, if independently, in very close connection with that institution. Many speakers have emphasised the necessity of keeping the measurement in close contact with the manufacture of gauges, and the further necessity of keeping research in contact with measurement; so that the manufacture, measurement and research on gauges should all be in close contact with each other.

Throughout the discussion I have been very much struck by the familiar and almost affectionate way in which people have spoken of the National Physical Laboratory as the "N.P.L.," and I am sure Sir Richard Glazebrook feels the compliment paid to the institution in the use of that abbreviation.

Lastly, I should like to thank Sir Richard Glazebrook and all the speakers in the discussion; also Dr. H. S. Allen, our secretary, and Dr. P. E. Shaw, who have taken so much trouble in organising it.

An exhibit of optical scales and graticules by Mr. J. Rheinberg was on view, and in closing the meeting the President called attention to it.

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A New Colour Transparency Process for Illustrating Scientific Lectures. By A. E. Bawtree, *F.R.P.S.*

Methods of illustrating scientific lectures may be divided into three classes: Blackboard drawings and wall diagrams, experiments and optical lantern slides. The first is unsatisfactory, especially before large audiences; the second entails much preparation and some risk of failure, and is limited in its usefulness; the third method is of far the widest application. For astronomical, microscopic and archæological subjects and for radiographs and other purely photographic records, monochrome photographic slides leave nothing to be desired, but for many purposes colour is necessary. Owing to the large scale of magnification upon the screen, only the crudest washes can be obtained by hand painting. Processes of natural colour photography are excellent when there is no very fine line work which would be destroyed by the grain of the image; but some subjects require definition of microscopic fineness combined with brilliant colouring. It was in order to illustrate a Paper before the Engineering Section of the British Association upon a method of bank note engraving that I devised the process illustrated.

The image is produced in a thin colloid film upon bare glass. Considerable experimenting led to the selection of a range of dyes and mordants by which practically any shade of the most brilliant colouring could be obtained. By suitable insulating films, images in any number of colours can be superimposed and accurately registered with one another. Thus, diagrammatic slides can be prepared in various colours. The passage of a beam of white light through a prism can be shown spreading out into bands of colour, instead of merely initialled lines. Coloured mosaics can be placed in a diffusing lantern to show the preparation of additive colours—*e.g.*, red and green producing yellow, more convincingly and brilliantly than with the Maxwell disc.

The process has other applications than the preparation of lantern slides. If a screen of lines ruled in alternate red and green of, say, 250 lines per inch, have projected upon it by means of a lens the image of a screen of similar ruling, but in lines of alternate black and white, the former screen will appear to change colour with a movement of the latter image of only 1/250 of an inch. Thus, this small motion can be rendered strongly visible to the largest audience. In this

manner compressive or tensile strain in a rod or the expansion of a rod with heat can be shown. By means of a balance beam fitted with a scale pan at one end and a bell crank at the other end, very small pressures—say 10 milligrams—can be made to act upon a heavy body; for example, a hollow leaden ball of 10 kilos weight just floating in water. By such an apparatus, combined with the two screens, the laws of fluid friction and of motion can be beautifully illustrated. In moving one inch, the ball will cause the screen to change colour 250 times, and these colour changes can be plotted against time in a variety of experiments. Numerous other applications of the method of preparing any form of colour transparency will doubtless suggest themselves to lecturers, and the process should place a useful additional means of demonstration in their hands.

XVII. *Absolute Scales of Pressure and Temperature.* By F. J. W. Whipple, *M.A.*

Received March 28, 1919.

It has been suggested to me that it would be fitting to call the attention of members of the Physical Society to the adoption by meteorologists of new scales of pressure and temperature, and to canvass the advantages of bringing such into general use.

The fundamental reason for adopting an absolute dynamical unit for pressure lies in the fact that the variation of gravity from place to place on the surface of the globe is quite appreciable. In practical meteorology pressures are required accurate to one part in 10,000, so that the allowance for this variation, which is of the order $\frac{1}{2}$ per cent., cannot legitimately be ignored. For many years it was customary to publish values of pressure in terms of the height of the barometer, making no allowance for this variation. In the meteorological charts, isobars were drawn to pass, not through places where the pressure was the same, but through places where the "head" of mercury was the same. A later development (which occurred in the British service in 1912) was to "reduce" the barometric readings to latitude 45°, the unit of pressure being the pressure due to an inch of mercury in latitude 45°. Such a unit is very artificial and is also irrelevant. The only satisfactory way to avoid the difficulty is to use absolute units which can be defined simply in terms of force and area.

The fundamental unit which has actually been adopted is the bar, the pressure due to a million dynes per square centimetre. The practical unit for meteorological work is the millibar, or 1,000 dynes per square centimetre. This unit is rather more than the gravitance of 1 gramme per square centimetre; it would be exactly the gravitance of 1 gramme per square centimetre at a place where the acceleration due to gravity was 1,000 cm./sec.^{2}. It is worth noticing that the millibar would be the pressure due to 1 cm. of water at maximum density at a place where gravity had this value.

It is necessary to emphasise the fact that the bar is not the same as the standard atmosphere used hitherto by the chemist, 760 mm. of mercury at 0°C. in latitude 45°. Actually the

bar is equivalent to 750·076 mm. of mercury under those conditions.*

The millibar is now in use, not only throughout the British Meteorological Service, but also in France. Other countries are gradually adopting it. It should be mentioned, however, that a different nomenclature is advocated by Prof. McAdie, of Blue Hill Observatory, which is attached to Harvard University. He wishes the name bar to be utilised for 1 dyne/cm.^{2}, so that our millibar would become the kilobar. He bases his advocacy on the tentative practice of a few chemists, but the weight of practical experience is in favour of the larger fundamental unit.

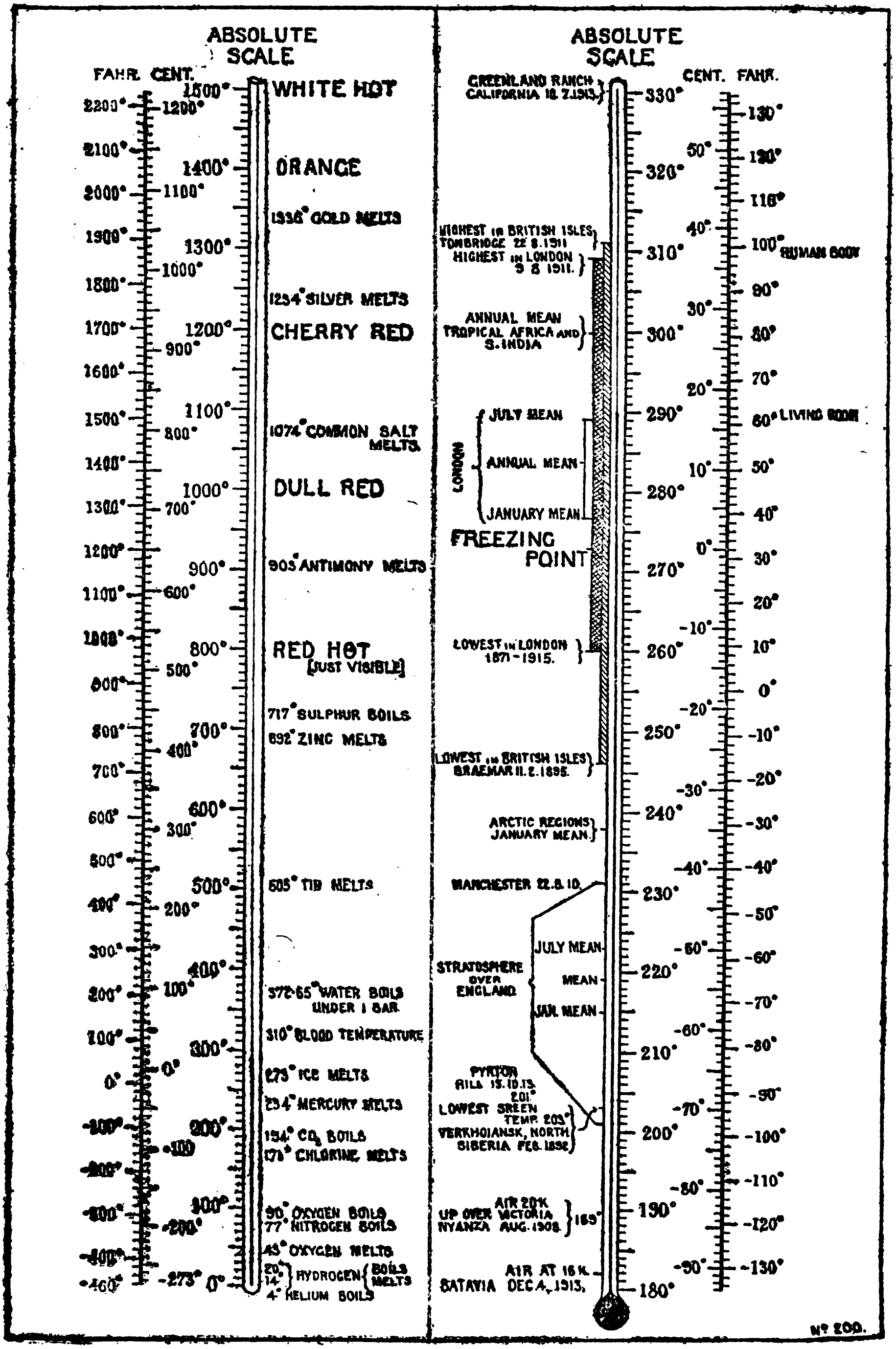
Instrument makers are now familiar with the graduation of barometers in millibars, and it is to be hoped that such instruments will shortly be introduced into all physical laboratories.

Another movement which was inaugurated about the same time as the first introduction of the millibar was that in favour of the everyday use of the absolute scale of temperature. The customary scales of temperature involve the frequent introduction of negative temperatures which are thoroughly illogical. With a scale such as the centigrade we are met with the paradox that $-272°$C. has a definite meaning, whilst $-274°$C. is unthinkable. It is hardly necessary to point out to the Physical Society that in thermodynamics it is always absolute temperature which counts. In meteorology we have to consider certain practical aspects of the question. In the first place, as may be seen in the accompanying diagram, the range of temperature with which modern meteorology, whose province is the whole atmosphere, is concerned goes from† 180a to 330a ($-130°$F. to 130°F.), so that in many investigations negative temperatures would abound if the Fahrenheit or centigrade scales were used; and in the second place, on the theoretical side, the application of the laws of gases are so frequent that the use of the absolute scale leads to a direct economy of thought.

The temperature scale which has actually been adopted by meteorologists is derived from the centigrade by the addition

* This relation is derived from the equations $g_{45}=980{\cdot}617$ cm./sec.^{2}; density of mercury at normal freezing point of water$=13{\cdot}5955$ gm./cm.^{3} Conversion tables for pressures will be found in the Computer's Handbook of the Meteorological Office.

† Sir Napier Shaw advocates writing 180a rather than 180°A on the ground that the degree symbol ° should be reserved for ang'es.



TEMPERATURE SCALES IN PHYSICS AND METEOROLOGY.
(Reproduced by permission from "The Weather of the British Coasts.")

of exactly 273°. This scale is not, strictly speaking, identical with the absolute centigrade scale, since the position of the absolute zero is not precisely −273·000°C. The meteorologist's scale has been called by some the pseudo-absolute scale. Sir Napier Shaw has introduced the name tercentesimal scale, on the ground that 300a falls within the range of temperatures to which we are accustomed.

Authorities differ as to the position of the absolute zero on the centigrade scale. The determinations quoted by Kaye and Laby ("Physical and Chemical Constants," 3rd edition, 1918, p. 44) vary from −273·05 to −273·27. The general mean is given by these authors as −273·13.

Accepting this mean result for the present, we see that on the true centigrade-absolute scale normal freezing point is 273·13, whilst the same point is 273·00 on the pseudo-absolute scale. The few certificates issued by the National Physical Laboratory

——	Absolute centigrade.	Pseudo-absolute.	I.F.P.
Absolute zero	0	−0·13	0
F.P. of mercury	234·26	234·13	234·15
F.P. of water	273·13	273·00	273·00
B.P. of water under 1013·2 mb.	373·13	373·00	372·95
,, ,, 1015·1 mb.	373·18	373·05	373·00
,, ,, 1000·0 mb.	372·78	372·65	372·60
B.P. of sulphur	717·8	717·7	717·5

for absolute thermometers refer, I believe, to the pseudo-absolute scale, though there is no explicit evidence to that effect on them. The convenience of the adoption of 273a for the freezing point rather than 273·13a cannot be gainsaid, and it may be desirable to regularise the position by defining a slightly modified scale of temperature such that 0 is the absolute zero and 273 is the normal freezing point. The degree of this new scale, which may be designated the I.F.P., or integral-freezing-point scale, provisionally, will be slightly less than the centigrade degree. The difference is so small, however, that hardly any measurements will be affected appreciably. For example, throughout the range of temperature of ordinary meteorological thermometers the departure of the I.F.P. from the pseudo-absolute scale is less than 0·03°C. Even at the boiling point of water under standard conditions the discrepancy between these two scales is only 0·05°C., and, therefore, only a third of the uniform dis-

crepancy between the absolute centigrade and the pseudo-absolute scales.

The I.F.P. scale has for its fundamental fixed points zero and 273. The boiling point under any specified pressure may be regarded as a secondary fixed point and used for calibrating thermometers. As will be seen from the table, the temperature 373 may be fixed as the boiling point under 1,015·1 mb.

The general adoption of the absolute scale for popular use in the near future is not to be anticipated, but there seems to be no good reason why it should not find a place in the physical laboratory at once. Thermometers are always being broken and replaced, and therefore the substitution of the absolute for the centigrade scale may well be a gradual process.

ABSTRACT.

The Paper urges the general use of the new scales of pressure and temperature which have been adopted by meteorologists. In the pressure scale the fundamental unit is the bar, the pressure due to a million dynes per square centimetre. The practical unit is the millibar. The temperature scale is that known as the pseudo-absolute scale, obtained by adding 273 to the centigrade scale. The author, however, considers that it would be advantageous to use the "Integral Freezing Point" scale, in which the interval between absolute zero and the freezing point of water is divided into 273 degrees exactly.

DISCUSSION.

Dr. C. Chree thought the millibar was a convenient unit for practical use He did not think too much stress should be put on the existing estimates of the absolute zero, which might be in error by more than was expected. He remembered how at one time the temperature 62°F. was fixed in reference to a particular Kew thermometer made of an unusual kind of glass, and used horizontally although it had been calibrated vertically.

Mr. F. E. Smith said that in reference to the remark about National Physical Laboratory certificates, these certificates always referred to the hydrogen scale, and there was in this case no ambiguity as between absolute or psuedo absolute.

Mr. Whipple said he had not noticed that any of the certificates he had seen specified the zero.

Prof. Lees asked if the author was definitely recommending the use of the I.F.P. scale rather than the other.

Mr. Whipple: Yes.

Prof. Lees then said that it was easy to get the correction from the pseudo absolute to the absolute centigrade scale by simple addition; but if the I.F.P. scale were adopted the conversion would be somewhat difficult, especially if the accepted value of absolute zero had ever to be revised.

XVIII. *On the Transmission of Speech by Light.* *By* A. O. Rankine, *D.Sc., Fellow of and Assistant in the Department of Physics in University College, London.*

Communicated by Prof. W. H. Bragg, F.R.S.

Received April 15, 1919.

Note.—The experiments described in this Paper were carried out between February and October, 1916, at the request of the Admiralty Board of Invention and Research. The results are now published by permission of the Admiralty.

Introduction.

The notable property of selenium of varying its electrical conductivity when exposed to illumination of various intensities has long been well known. It has led to various attempts being made during the last thirty or forty years to transmit speech over considerable distances by means of a beam of light which fluctuates in intensity in a suitable manner. Given such a beam of light the mode of reproduction is simple. A circuit is made consisting of a selenium cell exposed to the beam, a telephone receiver, and an electric battery. If the intensity of the beam does not vary, the electric current through the telephone receiver remains constant. But, if there have been impressed on the beam fluctuations of intensity corresponding in amplitude and frequency to the vibrations of speech or other sounds, the selenium, if it is capable of adjusting its conductivity with sufficient rapidity, will control the current in the telephone in such a way as to reproduce the original sounds.

This Paper is concerned chiefly with the manner in which it is possible to produce a beam of light, fluctuating in accordance with speech sounds. The methods hitherto used may be divided into two classes. In the first, the aim is to cause the speech to control the illuminating power of the source itself. For example, if the current in an electric arc can be controlled effectively by microphonic action, the light issuing from the arc may be expected to have the character desired. The second general method is to effect the control of the beam by causing the speech to interrupt the light, with the proper periodicity and amplitude, *after* it has left the source, the actual illuminating power of which remains constant.

Of these two modes, there is little doubt that the second is the more effective and useful. In the first place, it permits the use of the sun's light as the source, whereas, in the other method, artificial sources only can be used. This, it will be seen later, is of considerable importance. It is desirable, also, that the changes of intensity brought about by the speech should be made as great as possible. In the case of an arc controlled microphonically, however, even if the current oscillates from zero to its maximum value—which is unlikely—the variations of light intensity must still be comparatively small at the frequencies in question. If we take the average frequency of speech sounds as about 500 per second, it means that the brightness of the arc must alternate between maximum and minimum every 1/500 second, during which time the actual variation of its temperature, and, therefore, of its brightness, must be small. Speech would thus impose no more than a ripple of small amplitude upon the already powerful beam of light emitted from the arc. On the other hand, by controlling the beam after it has left a constant source, it is possible, particularly by the method about to be described, to guarantee that the fluctuation of the beam traverses the widest possible range.

Both methods have been used with a certain amount of success. In a patent specification of 1889, Graham Bell describes two devices falling under the latter class. In the first, the proposal is to allow the beam of light to pass in succession through two grids consisting of equal parallel strips alternately opaque and transparent. One of these is fixed in position, and the other is moved bodily, in a direction perpendicular to the strips, by the operation of a diaphragm to which it is rigidly attached, and on which speech sounds fall. The movements of the diaphragm may be expected, therefore, to control the obstruction to the beam of light in such a way that the intensity of the emergent beam varies in accordance with the speech sounds. The practical objection to this device is that, ordinarily, the movements of the diaphragm are so small that it would be most difficult, if not impossible, to make grids at once so light, so rigid and so fine as to fulfil the necessary conditions. Graham Bell does not, in fact, claim that this method has been actually used. His second device is much more sound from a practical point of view. It relies upon the fact that a vibrating diaphragm is continually altering its curvature; consequently, if polished, and

interposed in a steady beam of light, it will give rise to a reflected beam which is of variable divergence. Although the total amount of light reflected is actually unaltered, the fraction of it incident at a distance upon a receiver, not large enough to include the whole beam, will have a value controlled by the vibrations of the diaphragm. The objection in this case is that the diaphragm must be large in order to deal with a large quantity of light; for, if it is placed at a point where the beam has been concentrated to a focus, the effect of its changes of curvature upon the divergence of the beam is negligible. Further, from an acoustical point of view, it would be necessary for the diaphragm to be thin, and it would, therefore, not be suitable for adaptation as a mirror of good optical properties.

In January, 1916, Prof. W. H. Bragg asked the author to investigate the problem of producing a device for controlling by human speech the intensity of a beam of light, and at the same time suggested the former of Graham Bell's methods, being, however, unaware that the device had been proposed previously. The method about to be described was the outcome of this suggestion. It has proved to be a practical and reliable means of transmitting speech by light, and ranges up to 1½ miles have been achieved, in spite of the fact that, owing to war conditions, it has not yet been possible to construct the apparatus in its most convenient and efficient form. There appears, also, to be room for considerable improvement in the selenium cells which have been used as receivers, and it is quite probable that the range can be increased to several miles.

Description of the Apparatus.

In this apparatus the essential point is the substitution of the *image* of a grid for the material grid itself. This substitution at once surmounts all the difficulties inherent in Graham Bell's first proposal. It is no longer necessary to attempt to construct diminutive grids suitable for being mounted upon, and operated by, speech-receiving diaphragms. *Both* grids are now fixed in position, and may be of practically any size. It is the *image* of the first grid which is caused by the incident speech to move to and fro with respect to the second grid. The principle involved will be readily seen by considering the first diagram (Fig. 1). *A* is a small mirror—an ordinary galvanometer mirror of about 1 cm. diameter—

which can be caused, by means to be described later, to execute small angular oscillations about an axis perpendicular to the plane of the diagram. Light from a source S is concentrated upon it by means of the lens L—*i.e.*, an image of the source is formed upon it. The equilibrium position of A is arranged to be such that the divergent reflected beam proceeds to a second lens L_2, by which it is brought to a focus at F. A grid G_1, consisting of parallel strips alternately transparent and opaque and perpendicular to the plane of the diagram, is interposed in the position shown, close to the lens L_1; and a second grid G_2 of dimensions equal to the first is placed in the path of the beam reflected from A in a position such that $AG_2 = AG_1$. The small mirror A is a concave one of radius AG_1. Consequently, leaving out of account for the moment the effect of obliquity, a real image of the grid G_1 is formed

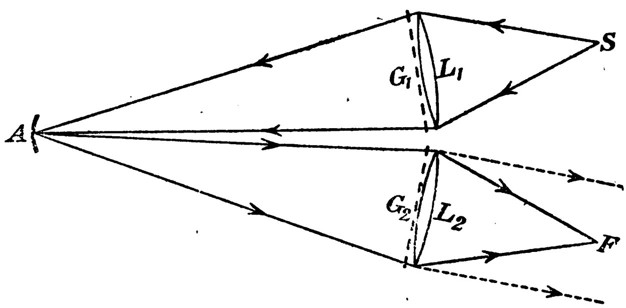


FIG. 1.

in the plane of the grid G_2. It will be noticed that, by reason of the fact that an image of the source of light is formed upon the mirror A, the curvature of the latter exercises no control on the divergence of the beam as a whole; what it does is to secure that *all* the light passing through any point in the plane of the first grid passes also through a corresponding point in the plane of the second one. It is clear that for a certain angular position of A the image of the first grid will be so situated on the second grid that, being of equal dimensions, the images of the opaque portions will become exactly superimposed on the transparent portions of the second grid. In this case no light at all will emerge from the system, to be focussed at F. For another position of A, the non-luminous portions of the grid image fall exactly on the opaque portions of the second grid, and the luminous portions on the trans-

parent slots; so that, apart from absorption, 50 per cent. of the original beam of light is brought to a focus at F. In general, A will be so placed that a fraction of the light varying between 0 and 50 per cent. of the original amount will penetrate the complete system. It will now be clear that if A has imposed upon it angular oscillations of any frequency and amplitude whatever, provided only that the amplitude is never greater than that corresponding to the width of one slot of the grids, the emergent beam will have a total intensity which is equal in frequency to, and proportional to the amplitude of, the oscillations of A. The image of the first grid, in moving to and fro over the second grid, in effect opens and closes a shutter in the path of the beam.

It is important to notice that for efficient interruption it is essential that the small mirror A should be a concave one, producing on G_2 a sharply defined image of G_1. Any source of light, if it emits luminous energy at all, has, of necessity, finite size. If A were plane, only a single point of the source would be provided for; to provide similarly for the *whole* of the light, the two grids must be at conjugate points with respect to A. It is not necessary, of course, that the grids should be equi-distant from A; but if their distances are not equal, they must still be such that the image of G_1 is focussed on G_2, and, in addition, the dimensions of G_2 must be adjusted so as to be identical with those of the image of G_1.

There are several ways in which the small mirror A may have imparted to it oscillations corresponding to human speech. Experience so far shows that, perhaps, the most direct one is the best. The mirror is attached rigidly to the lever which ordinarily carries the needle in a good quality sound box or gramophone recorder. The manner of attachment is indicated in the diagram (Fig. 2). Speech sounds traversing the conical trumpet impinge on the mica diaphragm, causing the lever to vibrate and to impart its angular motion to the mirror. Only small angular oscillations are possible by this means, the order being about 1/400 radian. But it is easy to see that by making the distances AG_1 and AG_2 (Fig. 1) large, and the spacings of the grids themselves of comparatively small dimensions, the speech sounds can be made to control efficiently the movements of the grid image, and, hence, the quantity of light emerging from the system.

A selenium cell placed at F so as to receive the emergent light, and, connected in series with a suitable telephone receiver and

battery, serves as the means of reproducing sounds corresponding to the fluctuations of the light. For speech sounds, with sound box and selenium cell of good quality, the articulation of the reproduced speech is extraordinarily good. It is interesting to note in passing, however, a striking effect which can be anticipated and which does actually occur in practice. Suppose that the small mirror A has imparted to it simple harmonic oscillations of a definite frequency n, but of variable amplitude. A tuning fork as the source of sound might, for example, be used for the purpose. If the amplitude is so small that the extreme movements of the grid image do not extend to more than the width of one space of the grid G_2, the intensity of the emergent light has the same frequency n, and this frequency is reproduced in the telephone receiver. An in-

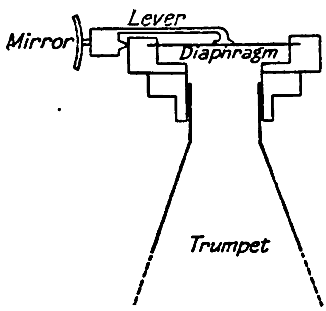


FIG. 2.

creased amplitude will, however, result in the light suffering interruption *twice* for each complete oscillation of the mirror, and the octave $2n$, in addition to the original note, will be produced. With still greater amplitudes the successive harmonics $3n$, $4n$, &c., will be added, and the original note may thus give rise to a whole series of overtones. When transmitting speech this effect makes itself evident by transforming an ordinary word into a screech. It can be avoided by modulating the voice so that the grid image, which is visible to the speaker, does not move over an excessive range. This is found to apply much more to some words than to others. "Four" and "five," for example, are very effective in their action on the diaphragm, and must be spoken comparatively softly; "two" and "three" produce feeble effects, and may be spoken more loudly.

When it is desired to project the fluctuating beam effectively to a distance, it is necessary that the vibrating mirror A should be at the focus of the optical system—in this case of the lens L_2. The light from every point of the source is then made into a parallel beam, as indicated by the dotted lines in Fig. 1, and the whole beam spreads at an angle determined by the ratio of the diameter of the small mirror A (supposing this mirror is completely covered with light from the source) to the length AL_2. The amount of light received on a limited area diminishes as the area is removed further from the projector. A lens or mirror of as large an aperture as convenient is placed so as to receive part of the projected beam, and the selenium cell is placed near the focus. The arrangement for this purpose is shown in Fig. 3, where also

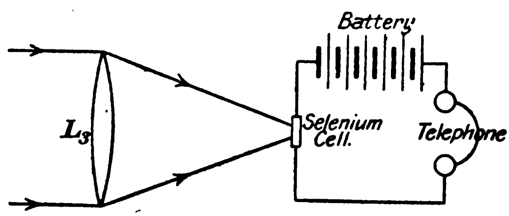


FIG. 3.

are represented the electrical arrangements for listening. It turns out to be best not to put the selenium cell precisely at the focus of the collecting lens L_3, but to place it so that the converging beam just covers the whole of the sensitive area of the selenium.

There are many alternatives in the optical arrangements of both the transmitting and receiving systems, and in sources of light. The question of the relative advantages of such factors will be dealt with later. One particular arrangement of the transmitting system (probably not the best) is shown in the photograph (Fig. 4). This particular apparatus has been made up from stock sizes of the various parts, owing to the difficulty of getting optical apparatus made to specification. It is shown set up for use with the sun as source of light. The large sun reflector—on the left—is a plane mirror of 9 in. diameter. It projects the light received from the sun, first through a water cell to remove most of the thermal radiation, then through the first lens and grid, so that an image of the sun is focussed on the small oscillating mirror,

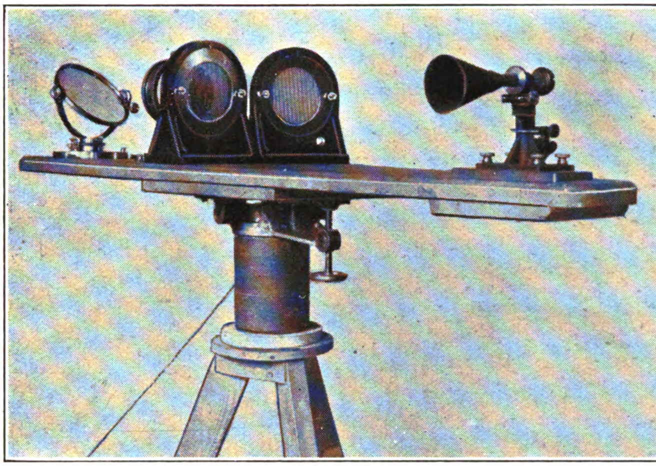


Fig. 4.

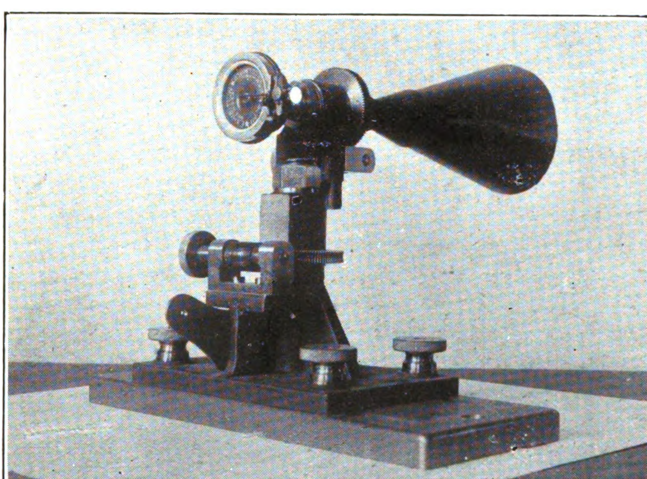


Fig. 5.

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which is actuated by sounds spoken through the conical trumpet on the right. The light diverges and passes through the second grid and lens, and is projected as a parallel beam to a distance. Each of the lenses has a focal length of 1 yard, and the radius of curvature of the small oscillating mirror is also 1 yard. The lenses and the grids have apertures of about $5\frac{1}{2}$ in., and the alternate opaque and transparent bands are each 0·1 in. wide. The apparatus has an altazimuth mounting and is provided with sights for directing it on to the receiving station. The sun reflector must be kept in continuous adjustment so that the image of the sun remains on the oscillating mirror. Artificial light can also be used. In this case the source takes the place of the sun reflector, and a lens of appropriate focal length has to be substituted for that used with the sun in order to produce an image of the source upon the oscillating mirror.

The mounting of the sound box and oscillating mirror is shown in greater detail in the photograph (Fig. 5). Provision is made for screw adjustment of the oscillating mirror about vertical and horizontal axes for setting the image of the first grid in correct position on the second. The trumpet is made of thick drawing paper, this having been found much superior to tin, which imparts metallic characteristics to the speech sounds.

With the above apparatus as transmitter and with selenium cells of the type available as receivers, speech is audible at considerable distances. Although the collecting lens used was only 7 in. in diameter, a "pointolite" lamp suffices up to a distance of half a mile. With a carbon arc as source, the range is considerably greater; whilst the sun multiplies the distance many times, probably to several miles.

Alternative Arrangements of Apparatus.

It is not proposed to enter fully into the description of the considerable variety of possible alternative arrangements embodying the principle of the apparatus already described. Several of these have been tried successfully. In the transmitting system, for example, it may be convenient to substitute for the lens L_2 (Fig. 1) a concave mirror of equal focal length. Other variations will be sufficiently obvious. An auto-collimating arrangement deserves especial mention, for most of its features seem to be ideal, at any rate in the case where the source of light is artificial. It enables one of the

grids to be dispensed with, and the beam of light is interrupted by means of the movements of the image of a single grid upon that grid itself.

A plano-convex lens, silvered internally upon the flat face behaves as a concave mirror, the radius of curvature of which is equal to the focal length of the lens. If, instead of silvering the whole of the flat face, we silver it only in parallel strips separated by equally wide unsilvered spaces, it will combine the properties of a lens with those of a mirror. Light incident upon its curved surface will be divided into two practically equal portions, one part being transmitted, and the other part reflected. Suppose we have such a lens (illustrated in section PQ in Fig. 6), and a source of light S removed a little from the axis OX of the lens, but in the focal plane. Half of the incident beam PSQ will be transmitted through the transparent spaces of the lens, and projected as a parallel beam represented by the lines PP_1, QQ_1, making with the

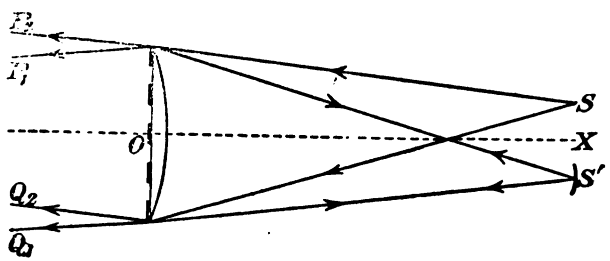


Fig. 6.

axis of the lens an angle equal to SOX. With this beam we have no further concern. The remainder of the beam PSQ, on the other hand, will be reflected by the silvered strips, and will converge to a focus at S^1. An image of S will thus be formed symmetrically placed with S itself relatively to the axis of the lens, both S and S^1 being in the focal plane. Now, if a small concave mirror whose radius of curvature is equal to S^1O be placed at S^1, and be actuated by a sound box in the manner already described, it can be used to produce a real image of the silvered grid upon this grid itself. The movements of the small mirror at S^1 thus determine the quantity of light which penetrates this grid, and a parallel beam of fluctuating character (represented by the lines PP_2, QQ_2) is projected in a direction making an angle equal to S^1OX with the axis of the lens, and twice

this angle with the non-fluctuating beam PP_1, QQ_1. It is easy to arrange that these two beams separate sufficiently rapidly to be entirely distinct at considerable distances; and it is merely a matter of adjusting the sighting arrangement accurately to guarantee that it is the fluctuating beam which reaches the point desired.

This arrangement works quite satisfactorily. It was, however, a difficult matter to make an accurate grid by hand, the method being to cut and remove strips from the lens surface which had been originally silvered all over. Suggestions for the more efficient construction of this type of lens are given later, and there appears to be little doubt that this arrangement will ultimately be superior to the other alternatives.

Alternative to Gramophone Sound Box.

From some points of view it may be desirable that the speaker whose words are to be transmitted should not necessarily use the trumpet mounted on the apparatus as shown.

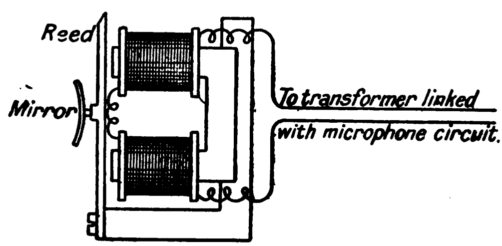


Fig. 7.

This may be provided for by substituting for the gramophone sound box a telephone receiver which is actuated by the variable current in a microphone to which the words are spoken. For this purpose a telephone receiver earpiece of the reed type is preferable, for the reed executes angular oscillations in response to alternating currents in the electromagnet. The small concave mirror (Fig. 2) is now attached rigidly to the reed of the telephone receiver, as shown in Fig. 7. The coils of the receiver are connected to the secondary of a suitable transformer, the primary of which is in series with a battery and the microphone. Sounds impinging on the diaphragm of the latter thus result in movements of the mirror which control the intensity and frequency of the light projected. This

arrangement has been tried and proved successful. In this case the microphone and speaker may be quite remote from the light transmitter. It has been found, however, that the intervention of the additional electromagnetic transformation of the speech sounds, results in a considerable loss of perfection in articulation.

Conditions of Efficiency.

It is now proposed to discuss in detail the various factors which determine the efficiency of the transmission of speech by the means described. Selenium is, apparently, sensitive to all the constituent parts of light forming the visible spectrum. Hence, it is clearly desirable that as great a quantity of light of all possible wave-lengths should be transmitted to and collected at the receiving end, and that all this light should be effectively controlled in intensity and frequency by the speech sounds. The selenium cell, too, should be constructed so as to be as sensitive as possible to rapid fluctuations of light.

(*a*) *Source of Light.*—If we make the assumption that the small oscillating mirror is covered completely with light by the image of the source formed upon it, the amount of light transmitted (apart from the small amount of absorption by the various mirrors and lenses) is determined solely by the brilliancy of this image. If the light forming this image is collected from the source by a lens or mirror of fixed aperture, the brilliancy of the image and the light projected to a distance are proportional to the intrinsic brilliancy of the source. Experiments have been made with three different types of source—a "pointolite" lamp, a carbon arc, and the sun. A rough comparison has been made of the relative intrinsic brilliancies of the "pointolite" and the sun. The method used was to form an image of each close together, by means of two separate lenses of equal focal length, and to diminish the amount of light received from the sun by applying various diaphragms until the intensities of the images were judged to be equal. The intrinsic brilliancies of the two sources were then in the inverse proportion of the areas of the respective apertures. Some difficulty was experienced in the comparison, owing to the difference of colour of the two images, but it may be taken that the ratio 250 : 1 fairly represents the relative intrinsic brilliancies of the sun and the "pointolite." The carbon arc is intermediate between the two, but much nearer the latter. It has not yet been possible actually to

compare the ranges for transmission of speech attainable with these various sources. Neglecting absorption it may be expected that the range for the sun would be $\sqrt{250}$—*i.e.*, 16 times as great as that for the "pointolite." The carbon arc would probably give two or three times the range of the latter. Although the "pointolite" is the least efficient it has one great practical advantage. It is a source of unvarying position. The carbon arc requires continual adjustment, and it is difficult to keep the image of the hottest parts of the arc (*i.e.*, the positive crater) steadily on the oscillating mirror. The same objection applies, although not to the same extent, to the use of the sun as source. An efficient automatic heliostat would overcome this difficulty; and, in addition to the great range attainable, the use of the sun as source does not involve any electrical equipment at all being transported with the transmitting apparatus.

(*b*) *Optical System.*—In the transmitting apparatus the two important constituents of the optical system are the oscillating mirror and the projecting lens or mirror. If conditions are arranged so that both are filled with light, the total quantity of light transmitted is determined by their apertures, and is in proportion to their areas. In the receiving system, the size of the receiving lens or mirror controls the amount of light collected and concentrated on the selenium cell. But it is not practicable to have it so large that it will collect at great distances more than a comparatively small fraction of the light projected by the transmitter. Consequently the diameter of the oscillating mirror, which merely controls the rate at which the projected beam diverges—and not its intensity—is comparatively unimportant, except in relation to the special consequences of this divergence. For example, in the actual apparatus already described, the ratio of the diameter of the oscillating mirror to the focal length of the projecting lens is about 1/100, so that the divergence of the projected beam is 0·01 radian. The diameter of the illuminated disc at a distance of a mile is 18 yds, and only a small fraction is collected by a receiver of reasable dimensions. The diameter of the oscillating mirror might, therefore, be much smaller without reduction of efficiency; but such diminution would require greater accuracy in directing the transmitter.

The oscillating mirror should be as perfect as possible so as to produce a clearly defined image of the first grid. The ideal

condition is that there should be no diffused light, so that for the appropriate position of the grid image on the second grid, no light at all should be transmitted. Care has to be taken to mount this mirror so that it is free from strain. If it is silvered on the back surface, it is usually found that the faint image from the unsilvered front surface has a position somewhat different from that of the main image. This can be remedied by rotating the mirror about its principal axis until the two images are in alignment, although not actually superimposed.

The intensity of illumination at a distant point is directly proportional to the area of the projecting lens or mirror, supposing the latter to be optically perfect. The larger this projector is, the greater will be the range. Unwieldiness is the ultimate limitation. For, in addition to having a large aperture, the projector should, for two reasons, have a correspondingly great focal length. In the first place, the angular amplitude of the oscillating mirror produced by speech of ordinary intensity is very small (of the order of 1/10 deg.). For efficient interruption of the light the width of the grid spacings must be small, and the distance between the oscillating mirror and the second grid large. In practice it has been found difficult to make grids of sufficient accuracy less than 0·1 in. wide, and, in such circumstances, for speech without inordinate effort, the focal length of the projecting lens should not be less than 1 yd. In the second place, spherical aberration troubles can only be avoided by keeping fairly large the ratio of the focal length to the diameter of the projector. It may be that the ratio usually adopted in astronomical instruments (about 12 : 1) is unnecessarily large for the present purpose, although it may be mentioned that very good results have been obtained using an 8 in. reflector of 7 ft. focal length, kindly lent by the Royal Astronomical Society. But a ratio of not less than 6 : 1 is almost certainly necessary for efficient transmission. With a projector 12 in. in diameter, the length of the apparatus would be in excess of 6 ft.—a size already somewhat unwieldy, apart from special provision as regards mounting.

The question as to whether a lens or a mirror should constitute the projector is a debatable one. Both have been used. A mirror has the advantage that it is free from chromatic aberration, but its efficiency is reduced by the fact that either it must be mounted obliquely so that the projected beam may

not encounter the sound box and trumpet, or one must allow the amount of light projected to be reduced considerably by the intervention of these objects. An ordinary lens loses light by chromatic dispersion, although it has the advantage that there is no obstruction when mounted normally to the oscillating mirror. The experience of the author has led him to prefer the lens, particularly if it is corrected for chromatic effects, because it leads to the single grid system already referred to.

It is interesting to make a calculation showing what gain may be expected from the use of achromatic lenses. Let us suppose that the projecting lens is uncorrected and that the oscillating mirror is placed at its focus for the extreme red component of the spectrum. Let F be its focal length for this colour, D its diameter, and d the diameter of the oscillating mirror. Then at a distance x from the transmitting lens, the diameter of the projected beam (Δ) is given by

$$\Delta = D + x \cdot \frac{d}{F},$$

$\frac{d}{F}$ being the divergence of the beam, if the whole of the oscillating mirror is covered with light. At great distances D becomes negligible, so that

$$\Delta = x \cdot \frac{d}{F} \text{ approximately.}$$

This gives the inevitable normal divergence attributable to the size of the source. If the lens were perfectly achromatic the other components of the light would also be confined within this disc. As it is, however, each of these components has an additional divergence, owing to the variation with colour of the focal power of the lens. If the focal length for the violet extreme of the spectrum differs by δF from F, the additional divergence φ in this case is

$$\varphi = \frac{\delta F}{F} \cdot \frac{D}{F}.$$

$\frac{\delta F}{F}$ is the dispersive power of the material of the lens over the range of colour specified. Calling this P we have that Δ_1, the

total diameter of the disc over which the violet light is spread at a great distance x is represented by

$$\Delta_1=\frac{xd}{F}+\frac{xD}{F}\cdot P \text{ approximately.}$$

Whence
$$\frac{\Delta_1}{\Delta}=1+\frac{D}{d}\cdot P.$$

The ratio of the intensities of the violet and red components respectively is equal to

$$\frac{\Delta^2}{\Delta^2_1}=\frac{1}{\left(1+\frac{D}{d}\cdot P\right)^2}.$$

It will be seen that, especially if the ratio of the diameter of the projecting lens to that of the oscillating mirror is large, the violet light may be spread over a much greater area than the red, and, in consequence, be less effective on the selenium. In the apparatus previously described the projecting lens is of crown glass for which $P=0{\cdot}043$, and the ratio $\frac{D}{d}=17$.

Thus
$$\frac{\Delta^2}{\Delta^2_1}=\left(\frac{1}{1{\cdot}73}\right)^2=\tfrac{1}{3} \text{ approximately.}$$

For colours intermediate between the red and the violet, the reduction of efficiency above indicated will be less marked, but still considerable, and it is clear that the use of achromatic lenses would result in a notable increase of light on the selenium receiver.

In the case of the single-grid system, it is necessary that the strips should be reflectors. In the apparatus of this type which has been tried, the grid was formed by cutting and removing strips of silvering from the plane surface of the lens which had been previously silvered all over in the ordinary way. It deteriorated rather rapidly owing to the corrosion of the edges of the strips.

This might be avoided by depositing the film by kathodic bombardment through a template in the form of a grid. It would probably be advantageous to protect the grid by a sheet of plane glass attached by Canada Balsam. The author believes that an achromatic lens, of optical quality approaching that of a telescope objective, treated in the way above described, would form the most efficient form of the apparatus. Such a lens, say, of 8 in. aperture and 4 ft. focal length, com-

bined with a oscillating mirror of 4 ft. radius of curvature, would constitute a transmitter of convenient size.

The remaining part of the optical system is that at the receiving end. Here, either a lens or a mirror may be used, and either should have a large aperture in order to concentrate as large as possible a fraction of the transmitted beam on the selenium. Optical quality is not so important as in the transmitter, but it still deserves attention. It is significant that, in practice, it has been found that an ordinary 7 in. lens forms as efficient a receiver as a 24 in. searchlight mirror. Although in the former case the selenium cell was shielded from all light save the transmitted beam, and consequently, the cell itself operated more efficiently, the discrepancy cannot be wholly accounted for in this way. A large part of it is undoubtedly due to the inaccuracy of the reflecting surface of the searchlight mirror, resulting in the waste of a large fraction of the incident light.

(*c*) *Grids.*—Very careful attention has to be given to the construction of this part of the apparatus. For reasons already indicated it is necessary that the spacings should be quite narrow. Two things are essential. In the first place the image of the first grid must be of the same dimensions as the second grid. It is clear that if they are out of step by one spacing, the fluctuation of intensity of the whole emergent beam will be reduced to zero, for the beam will then consist of two equal halves fluctuating in opposite phases. A difference of even a small fraction of the width of a spacing is detrimental, although not to the same extent. The second essential is that the grid image and the second grid must be strictly parallel to one another. If the two intersect only once there is no resultant variation of the total light transmitted. It has been already pointed out that, theoretically, the two grids may occupy any pair of positions conjugate with respect to the oscillating concave mirror, provided their sizes are appropriate. But as a consequence of the above conditions, it has been found that the most satisfactory results are obtained with the distances from the oscillating mirror equal; for in this case, the two grids have to be equal in size. A pair may, therefore, be milled together out of superposed sheets of metal, so that, whatever may be the inaccuracies of milling, the two are of practically indentical shape. The second is used inverted with respect to the first, and coincides with its inverted image. This is only so with sufficient accu-

racy when the angle is small through which the whole beam has to be turned by the oscillating mirror in order to penetrate the projecting lens. Thus, in Fig. 1, if the angle G_1AG_2 is considerable, the light falls *obliquely* on A, and the image formed by this mirror is both reduced in size and nearer to A than it would have been for normal incidence. This is an additional reason for keeping the ratio of the diameter of the lenses to their focal lengths comparatively small. Should the effect of obliquity become serious, it may be largely reduced by arranging that the grids are parallel, instead of being perpendicular, to the plane G_1AG_2, and by providing that the oscillating mirror is caused to vibrate about the appropriate axis—*i.e.*, a horizontal one. The grids are then parallel to the secondary and not the primary focal line, and the position of the former is not so sensitive to increase of obliquity as in the latter case. This effect of obliquity disappears almost entirely in the case of the single grid system, since the light is parallel to the axis of the oscillating mirror.

However accurately the grids are made in practice their edges are bound to suffer from some degree of imperfection. The normal position of the oscillating mirror is arranged so that the images of the edges of the first grid coincide with the *middles* of the strips of the second grid. The intensity of the speech should then be modulated, so that the amplitude of vibration is never so great that coincidence of edges quite takes place. The transmitted beam, averaging 50 per cent. of that leaving the oscillating mirror, will then have an intensity which fluctuates in strict accordance with the vibrations of the diaphragm, even if the edges are inaccurate, provided the edges of image and second grid never cross. The *mean* intensity of the projected beam is the same whether it is fluctuating or not. The eye is, of course, unable to perceive the separate fluctuations of speech frequency, so that an observer would see an apparently steady light, and that only if he happened to be within the comparatively small angle of divergence of the beam.

To start with the grid image and second grid in precise register gives rise to three disadvantages. These have been encountered in practice, and it is easy to see the reasons for them. If one arranges that the normal amount of light transmitted is as near zero as possible—*i.e.*, if the images of the slots of the first grid are normally exactly superimposed on the opaque strips of the second—the accuracy of the edges

becomes important. Any departure from perfection results in the variation of intensity of the beam not being in strict proportion to the change of displacement of the oscillating diaphragm. This shows itself in a loss of articulation in the words transmitted. Further—and this is still more important in its effect on faithfulness of reproduction—each single vibration of the diaphragm produces *two* (instead of one) maxima of intensity in the projected beam, resulting in the formation of octave overtones in the sounds received. With this setting, also, every word spoken into the trumpet causes the mean intensity of the projected beam to be no longer zero ; successive flashes, corresponding to each syllable, are seen, and it is evident to an outside observer that signalling of some sort is proceeding. From all points of view, therefore, the "half" setting first described is definitely preferable.

The effect of excessive amplitude of vibration in relation to width of grid spacings has been already referred to. For speech of a constant intensity, the amplitude of the light fluctuations is increased by reducing the width of the grid spacings. Or, looked at from the other point of view, feeble speech sounds combined with sufficiently narrow spacings, can produce effects equal to those arising from louder speech with wider spacings. The spacings should be as narrow as is consistent with accurate construction. In such circumstances, and with a bright source of light, the transmitter may actually act as a relay, the sounds heard in the telephone receiver being more easily audible than those incident on the transmitter. This effect has been actually observed at considerable distances, when the sun has been used as source.

(*d*) *Sound Box.*—The sound boxes used have been those of the "Exhibition" grade, made by the Gramophone Company, Ltd. They appear to copy sounds most faithfully and have given excellent results. It has been suggested that for the present purpose a less stiff diaphragm might be used so as to make possible the use of grids with wider spacings, or, alternatively, to produce equal effects with feebler sounds It is very doubtful, however, whether the gain of this advantage would compensate for the possible loss of two others. Stress has already been laid on the importance of maintaining the "half" setting of the grid image, and this is best secured by a stiff mounting for the oscillating mirror. In addition, a stiff diaphragm is undoubtedly more nearly aperiodic for speech frequencies than a more flexible one, and is consequently a

more faithful reproducer. Altogether, the above sound boxes are so good that there is little room left for improvement. The articulation of the speech heard at the receiving end is already so clear that there is little or no difficulty in recognising the identity of a speaker whose voice is familiar to the listener. This fact proves not merely the excellence of the transmitting sound box, but also that of the telephone receivers which are in series with the selenium. These have been of the reed type manufactured by S. G. Brown, Ltd. The possibility of the substitution of one of these receivers (in conjunction with a microphone) for the sound box has already been mentioned. It is practically certain that the loss of articulation in this case is attributable to the microphone, and not to the telephone receiver.

(*e*) *General Mounting of the Apparatus.*—For convenience of transmitting it is clear that all the parts of the transmitter should be mounted on a rigid base which is capable of being rotated about horizontal and vertical axes. The mounting required is, in fact, similar to that necessary for a gun. For most purposes ordinary pin-hole sights give sufficient accuracy in directing the beam, but telescopic sights might be necessary for great distances. It is best to adjust the sights or telescope in practice by causing the projecting lens to form an image of a distant object on the oscillating mirror, upon which it can be readily seen even in daylight, and then setting the sights or telescope in line with the object. There is, of course, a good deal of latitude permissible in the sighting, depending on the divergence of the projected beam. An accuracy of only half a degree, for example, is necessary in the transmitter shown in Fig. 4. As it is *angular* accuracy only which counts, there is no increase in difficulty of pointing as the distance increases.

The receiving apparatus also should be similarly mounted. In a double set the act of sighting the transmitter would automatically put the receiver in adjustment for receiving.

(*f*) *Selenium Cells.*—The selenium cells used in these experiments have all been of the type due to and made by Dr. Fournier D'Albe. Those giving the best results have had a resistance (when not illuminated) of from 10^5 to 2×10^5 ohms. A pair of Brown receivers, wound to a total resistance of 6,000 or 8,000 ohms proved the most satisfactory. A transformer may be used so as to avoid any continuous

component of the current in the receivers, but this is somewhat detrimental to articulation—probably owing to the hysteresis of the iron core of the transformer. A satisfactory substitute for a transformer is the arrangement shown in Fig. 8. In series with the selenium cell is put a non-inductive resistance R, the resistance of which equals that of the selenium cell under the conditions of use. The ends of this resistance are tapped (through a condenser) by the telephone receivers. In this case variations of current only pass through the latter, and, by choosing the condenser so that its impedance for speech frequencies is very small compared with that of the telephone receivers, the articulation can be rendered not noticeably inferior to that in the direct arrangement.

There is no doubt that these selenium cells are capable of responding sufficiently faithfully to the rapid fluctuations of the transmitted beam to give surprisingly good articulation. They appear to retain this property over long periods, particularly if not in regular use. Some of them, manufactured three years ago, are still good, although not probably quite so efficient as they were originally. Constant exposure to bright light does, however, produce slow deterioration, especially in combination with a high voltage. The limit permissible is apparently about 40 volts; otherwise the cell rapidly breaks down and ultimately becomes practically non-conducting. It is significant that the above voltage is the arcing potential of carbon, and it seems quite likely that, above this voltage, arcing between the graphite layers forming the basis of the cell sets in and burns out the selenium.

There is also evidence that the cells work better when shielded from bright extraneous light; and a cell, which it is desired to keep especially sensitive, should be reserved for use at great distances, when the amount of light falling on it will always be small.

Although these selenium cells have given admirable results, it is certain that there is room for great improvement in their sensitivity to *rapidly fluctuating* light. This is very important, since it limits the range obtainable with the present method of transmitting speech by light. At telephonic frequencies the cells do not respond as quickly as might be to the changes of intensity of the incident light. Suppose that a selenium cell is in circuit with a battery, and that it is exposed to light which can be interrupted at any desired rate. There will be an

alternating current superimposed upon the steady current already in the circuit. So long as the speed of interruption is low, say, 1 or 2 per second, the amplitude of the alternating current will be comparatively large. But this amplitude diminishes very rapidly as the frequency of interruption is increased. The result is that the alternating current obtained at speech frequencies is not nearly so large as it would have been in the complete absence of lag. A good selenium cell whose resistance in the dark is, say, 110,000 ohms, will have its effective resistance reduced by 20,000 ohms (*i.e.*, to 90,000 ohms) by continued exposure to a source of 50 c.p. 1 metre away. Let this cell be in series with a battery of 30 volts and a pair of telephone receivers of 6,000 ohms. The steady current, if the cell is kept dark, will be 30/116,000 amperes=259 microamperes, while that during the exposure of the cell to the light will be 30/96,000 amperes=312 microamperes. Thus, if alternations between light and darkness occur very slowly indeed, the alternating current executes an amplitude of 312−259=50 microamperes approximately. In the complete absence of lag in the selenium, even at speech frequencies (say, an average of 800 per second) the amplitude of the alternating current would still be very considerable, although it would be reduced by the increased impedance of the telephones. These would have at such frequencies an impedance of about 100,000 ohms, so that the current amplitude would be reduced to about 25 microamperes. Such an alternating current as this would produce sounds of almost unbearable intensity in such telephones, which require only a fraction of a microampere to give audible effects. The sounds heard would actually be just about at the lower limit of audibility, proving that the time lag in the selenium has effected a reduction of the current amplitude from 25 microamperes to a small fraction of 1 microampere. The fact that the articulation observed is actually so good is apparently accounted for by the lag being independent of frequency at high values of the latter. If the selenium cells could be so modified as to eliminate or reduce the lag, there would be a considerable augmentation of their efficiency, and of the practical range of transmission of speech. The author has no experience of the methods of manufacture of these cells, but suggests that a diminution of the thickness of the selenium layer might be beneficial.

(*g*) *Possibility of Amplification.*—Even if this improvement in the selenium cells should turn out not to be feasible, there

are still two other methods by which the range can be increased. The first is by increasing the apertures of the transmitting and receiving optical systems. The second is by the use of amplifiers to magnify the alternating currents flowing in the selenium circuit. Of these the second presents less difficulty. Large lenses or mirrors of good optical quality are at present made by processes which are both laborious and costly. Various attempts have been made to use thermionic valves with some success. It soon became evident, however, that this particular type of selenium cell is not capable of being used satisfactorily when the amplification is considerable. Extraneous noises interfere too much. Even when the selenium is kept unilluminated the current flowing in the cell is of an intermittent character which makes itself evident by a loud grating noise in the telephone receivers after triple amplification. This noise is practically inaudible

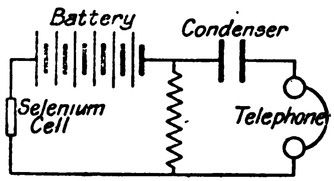


FIG. 8.

in the absence of amplification. It may be that this intermittency of current arises in the selenium itself, but it is more probable that it is caused by the graphite backing which forms the conductor to and from the selenium, since a precisely similar grating noise occurs after amplification of the current passing through a layer of graphite alone, *i.e.*, in the absence of selenium altogether. Whether the current can be rendered more continuous by the substitution of other substances for graphite is a matter for further experiment.

In order to use a valve amplifier with a selenium circuit, it is necessary to provide either a suitable transformer or an arrangement like that shown in Fig. 8, connecting the two telephone leads instead to the filament and grid respectively of the first valve. This is to prevent any voltage, other than the alternating one, operating on the grid and filament. Of the two arrangements the latter is much preferable. In an ordinary three-valve set there are three, or even four, transformers with iron cores. The result is

that in amplifying speech sounds, there is a very great deterioration of articulation, so much so that, although the sounds are very loud the actual words are often quite unintelligible. If, however, non-inductive resistances and condensers are substituted for the transformers, there is a very marked improvement, and this type of valve set has been found to work very satisfactorily with selenium cells, apart from the defect of the latter already referred to.

There appears to be very little doubt that, if selenium cells can be produced which are either more rapid in action, or free from intermittency of conduction except when induced by light fluctuations, the transmission of speech by light will be much facilitated. If both these improvements can be effected, even a comparatively feeble source would probably be capable of transmitting many miles. Already the ranges which have been obtained are quite considerable. With a carbon arc as source, and the projector an astronomical mirror the aperture of which had been reduced to 2 in., speech was quite audible at half a mile. Conversation in both directions has been carried on with 20-in. projectors of poor quality between stations $1\frac{1}{2}$ miles apart, even when considerable mist intervened. With the sun as source, instead of a carbon arc, the faintest whisper could be heard at this distance. All this was without amplification of the sounds received. With satisfactory amplification it is impossible as yet to anticipate how great the limiting range may be.

ABSTRACT.

Light from a point source is collected by a lens of about a metre focus, and an image formed on a small concave mirror, which is attached to the diaphragm of a gramophone recorder. The light diverges and passes through a second similar lens, which projects it to the distant station. Two similar grids are mounted, one in front of each lens. An image of the first grid is superposed on the second by reflection in the concave mirror. When the latter oscillates under the vibrations of speech, the dark spaces of the image move over the openings of the second grid, thus producing fluctuations of the intensity of the beam. The light is received by a collecting lens and focussed on a selenium cell in circuit with a battery and telephone receiver.

DISCUSSION.

Mr. E. H. Rayner thought it a pity that no advantage had been obtained by using a searchlight reflector. At present these mirrors were moulded, and the errors in focus were considerable. Sometimes, however, a really good one could be found by selection.

Prof. Fortescue admired the ingenuity of moving the image of a grid instead of the grid itself, as with any form of microphone the available forces

were small. It appeared that at present very little was known about the selenium cell. It was desirable that physicists should investigate this more fully.

Prof. PORTER commented on the author's thorough investigation of the conditions governing the design.

Mr. C. C. PATERSON asked how much of the sensitivity of the selenium cell was actually effective at these frequencies.

Dr. ECCLES called attention to the historical association of phototelephony with University College, London. Graham Bell's father was a member of the Physics Department at University College some forty years ago, and he himself, there is reason to believe, may have begun there his experiments on phototelephony. The author of the present Paper had worthily upheld the traditional position of the college in this subject. With regard to the present apparatus, the speaker took the opportunity of testifying to its excellent performance in the field. He had witnessed successful trials over a distance exceeding half a mile, and found the articulation much more perfect than is usually achieved with carbon microphones. There are two main types of phototelephony, one in which the source of light is kept constant and the flux of light along the beam modulated by the voice, and one in which the emission of light from the source is modulated. Dr. Rankine's method belongs to the former class. The well-known arc method is illustrative of the latter class, and has been stated to have been used on ships of the German Navy at sea over distances of 7 miles. Its chief disadvantage relative to the first type lies in its poor articulation (so far as it has been developed); its advantage arises mainly through the smallness of the influence of the tremors and shocks always present in a ship, for instance, against which the mobile mirror in Dr. Rankine's apparatus could be protected only with great difficulty. Nevertheless, as an improvement on Graham Bell's grid method, Dr. Rankine's conception of moving a light mirror instead of a heavy grid was an elegant and practical step.

Mr. T. SMITH: I have been greatly interested in Dr. Rankine's Paper, and wish to express my admiration for the way in which he has overcome the difficulties inherent in any problem involving the reproduction of effects at a considerable distance. I propose to limit any comments I have to make to the discussion of the optical problem involved. This is by no means an easy one, and the author is to be congratulated on the efficiency of the arrangement he has adopted. I am not, however, in agreement with much that he has said concerning the optical system in discussing the conditions of efficiency, and I consider that some of his conclusions need modification. Referring to Fig. 1 of the Paper, and disregarding for the time the grids, the optical system, no matter how it is constructed, produces an image of the extended source S. Neglecting absorption and reflexion losses, this image will be of the same intrinsic brightness as the source S itself. The lens L_2 may thus be looked upon simply as an aperture through which the image can be seen. As long as the image appears to fill the aperture of L_2, when viewed from the receiver—that is, as long as S is not too far removed from the focus of the projecting system as a whole, the correction of this system for chromatic and other aberrations is not of the least importance, and makes no difference in the efficiency of the apparatus. In fact, the aperture of the lens L_2 may for all practical purposes be regarded as itself a luminous source of light. Coming, now, to the receiving system, shown in Fig. 3, the lens L_3 produces an image of the luminous aperture L_2 near its principal focus. All the useful light lies within a cone bounded by the aperture L_3 and the image of the aperture L_2 formed by the lens L_3. It is evident that the efficiency of the apparatus is increased by enlarging the apertures of both the projecting and the receiving systems (involving, in the latter case, an increasing focal length) up to the point where the image of L_2 in L_3 just covers the sensitive area of the selenium cell. These considerations show that the colour correction of the projecting system is not of importance, but that the

correction of the receiving system becomes important when the image of the aperture of the projecting system approaches its maximum useful size. There appears, however, to be very little justification for indulging in very expensive lenses of large aperture in this particular application of optical instruments. The conditions are so totally different from those under which optical instruments are normally employed that the limitations which are such an inevitable barrier to the optician can be evaded. Instead of employing a relay to amplify the current variation in the microphone circuit, with the consequent objectionable grating sound that has been mentioned, it would appear preferable to use a number of receiving sets, each consisting of an optical system and a selenium cell, and to connect these in parallel. There would be no need to make any special selection of lenses, as the high velocity of light will prevent the perception of any departure from perfect agreement in phase. The duplication of the projecting apparatus with the diaphragm of the trumpet as a common element, though not by any means impossible, presents some difficulties. Reverting to the projection system, there is little doubt that if grids are employed they must be placed approximately in the positions shown in Fig. 1. If C is the centre of curvature of the mirror A the grids should lie on the sphere, having AC as a diameter. The mirror A, and preferably also the source S, should lie within the focus of the projection system, so that their images are virtual. This will prevent the occurrence of bad spots in the projected field due to blemishes on the surface of the mirror. There would probably be decided advantages in dispensing with the grids entirely and using some other means for varying the intensity of the light. One consequence of the use of the grid is an initial reduction in the intensity of the light of 50 per cent. The difficulty is that the variation in intensity must be accomplished by very minute movements of a small piece of the apparatus. It follows that this portion of the apparatus must be situated near an image of the light source, and since this is roughly conjugate to a distant point of the external field no mechanism of the comb type can be admitted. It appears, however, possible that a glass absorbing wedge with a very sharp rate of increase of absorption with distance would be admissible. This would preferably be arranged to move in a vertical plane with its edge horizontal, so that the apparatus could be set up accurately if a spirit level were attached to the base, the adjustment in a horizontal plane being carried out as at present. A stationary inverted wedge of similar gradation could be employed to give a uniform field. The mirror A would in this case be removed, and the whole apparatus moved into line. By removing the wedges and the arc and substituting an eyepiece near S the apparatus would be converted into an erecting telescope, and could be used for sighting with great accuracy when long ranges were in question. With such an arrangement the special difficulties consequent on the use of the grid disappear, and the projecting system can be made much more compact. There is no reason why in this part of the apparatus lenses should not be employed having very large apertures with short focal lengths. There is little doubt that the author is correct in preferring to use lenses in place of mirrors.

Mr. A. P. Trotter described some experiments which he had been invited by the Admiralty through Dr. Eccles to carry out. He did not know who the inventor was, or if more successful results had been obtained elsewhere. His own results were not worthy of more than a place in the discussion on Dr. Rankine's Paper. Almost all the apparatus had been provided by Dr. Eccles. Small lamps having tungsten filaments about 5 mm. in length were filled with hydrogen or with mixtures of hydrogen and nitrogen. The best were filled with hydrogen at pressures from 200 mm. to 600 mm. below atmospheric. A circuit was made through a microphone and a lamp, either direct or through a transformer, and the filament responded to the undulations sufficiently to reproduce speech when the light was concentrated on the selenium cell. The utmost distance that he could transmit speech with

lenses 76 mm. (3 in.) diameter was about 2 metres. The cells were about 8 mm. square.

Dr. Eccles said that the method of light telephony just alluded to was an invention of his own, which he had hoped to describe at a future date along with two or three other novel methods. The idea underlying the method alluded to is to obtain a glow lamp with so fine a filament that its temperature and light emission could follow acoustically produced alterations of filament current. The best telephonic results were obtained when the lamp bulbs were filled with hydrogen. After the method had been shown feasible for speech frequencies, some of the early test lamps were sent to Mr. Trotter, and he was asked to measure, if possible, the actual fraction of a second taken by the filament to rise to and fall from its maximum brilliancy after a current was switched full on or right off. This he accomplished very successfully. Regarding Mr. T. Smith's suggested use of a black glass wedge moved by the voice to produce modulation of the flux from a steady source, the speaker said he had used and found excellent a similar and even simpler method, in which a sewing needle fixed to the sound box of a phonograph had its shadow projected by aid of an obvious optical system upon the distant selenium; the acoustic vibrations of the needle caused a magnified motion of the shadow which covered and uncovered the selenium correspondingly, and gave as the result very perfect articulation in a telephone receiver in series with the selenium.

Mr. F. E. Smith asked to what extent the cells were screened, as the sensitivity varied greatly with the total illumination of the cells. He thought that the method might be of great use in the development of speaking and singing cinematograph pictures, as the record could be in the form of a second film working beside the other, producing variations in a second beam of light in exact synchronism with the picture. A receiving system as in Dr. Rankine's arrangement would convert these fluctuations into speech.

Mr. P. R. Coursey suggested the use of a polarised beam of light modified in intensity by electromagnetic rotation.

The Author, in reply to Mr. Paterson's questions, pointed out that the selenium cells used are not sufficiently constant for the measurement of light, but such constancy is not demanded of them in the present case. The sensitive area is about $\frac{1}{4}$ in. square, and the light received is purposely spread over this area, and not brought to a small focus. No precise measurements have been made, but it is certain that the sensitivity to slow fluctuations is much greater than for rapid changes, and it is doubtful whether more than one-thousandth of the full sensitivity is released at speech frequencies. My experiments are in agreement with Mr. F. E. Smith's comment that screening from extraneous light is advantageous—this is one reason why a lens receiver is preferable to a mirror receiver. His suggestion regarding synchronism in cinematography is very interesting. I have always contemplated obtaining photographic records of the intensity of the beam by means of a moving film camera, with a view to reproduction by selenium, but this possible application to keeping sounds and pictures in time is new to me.

I find it rather difficult to follow Mr. T. Smith's criticisms of the optical system. I would have preferred if he had shown in what respect my calculation of the advantage accruing from achromatism breaks down. Mr. Smith is an expert on optics, but I think in this case he is wrong. I see no escape from the argument that a definite quantity of light of all colours, which falls on the projecting lens is confined to a certain area at any given distance if the lens is properly corrected, but is spread over a larger area in the absence of correction. The amount of light received per unit area is consequently less in the latter case. Mr. Smith speaks of *the focus* of the projecting system. But the system has an infinite number of foci for polychromatic light, and, in terms of his argument, if S is at the focus for violet light it is *too far removed* from the focus for red light for his deduction to

remain valid. In addition to this I may call attention to three practical points: (i.) That the best results I have obtained have undoubtedly been with an astronomical mirror as the projector; (ii.) that the edges of the beam projected through an uncorrected lens are actually found to be coloured; and (iii.) that Prof. R. W. Wood, in a Paper recently read before this Society, described a chromatic lens which he had used for long-distance signalling, and which, presumably, he had found superior to uncorrected ones. The reason why corrected lenses are not so important in the receiver is that there is very considerable latitude in the position of the selenium cell. It is easy to arrange that the whole of the light, including the coloured edge, falls on the sensitive area of the selenium. With regard to the suggested use of a multiple receiver, this is, of course, possible, but the same effect is obtained by increasing appropriately the aperture of a single receiver. I am afraid I cannot agree with Mr. Smith's suggestion that it might be a decided advantage to dispense with the grids entirely, particularly if a system of two light-absorbing wedges, as suggested by him, is the alternative. The present method was devised in order to escape the great difficulty of operating any form of shutter or its equivalent by means of the minute movements of the speech in receiving diaphragm. Light absorbing wedges would surely, in effect, *diminish*, instead of increase, these movements in the ratio of the tangent of the angle of the wedge. It is perfectly true, also, that in the present system 50 per cent. of the original light is lost, but it is unlikely that a pair of wedges could be made to secure any improvement in this respect. Prof. Eccles has told us that he has used this system, but I doubt whether he would claim that it is as efficient as the device which is the subject of the present Paper.

The Use of the Triode Valve in Maintaining the Vibration of a Tuning Fork. *By* Prof. W. H. Eccles.

A TUNING FORK was exhibited sustained in vibration by means of a triode valve instead of by means of contacts. In the form described, two electromagnets act upon the prongs, and the windings of one magnet are in the grid circuit; those of the other magnet are in the plate circuit of the tube. When the fork is in motion the E.M.F. induced in the grid magnet controls the current flowing in the plate circuit and its magnet, with the result that the motion is sustained.

By increasing the distances between the poles of each magnet and the prong confronting it, the apparatus may be adjusted so that the fork is just not maintained in oscillation. In this condition the arrival of a train of sound waves of exactly the right frequency to produce resonance causes the fork to start into vibration. The instrument is then a tuned relay. A large fraction of a minute is required in some adjustments to provoke the full response.

DISCUSSION.

Mr. F. E. Smith said he had at one time been working on somewhat similar lines to Dr. Eccles, with a rather different object—viz., the accurate measurement of a very small time interval. He used first a purely electrical arrangement, and found that a very constant frequency (about 1,000 per second) could be obtained if the electrical conditions were right. For certain reasons, however, it was thought desirable to employ a tuning fork, and one of those had been borrowed from Prof. Eccles. He had found it essential to put a condenser across the inductance of one of the magnets to get approximate tuning in the electrical circuit, otherwise the fork would not respond readily. In his arrangement the currents induced by the vibration of the fork produced oscillations of the filament of an Einthoven galvanometer, the deflections of which were photographically recorded.

XIX. *A Form of Knudsen's Vacuum Manometer.* By LEWIS F. RICHARDSON.

RECEIVED APRIL 11, 1919.

IN 1911 the author was in need of a vacuum gauge for measuring pressures of the order of 1 dyne $cm.^{-2}$, or less, in electric lamp bulbs for the Sunbeam Lamp Co. The McLeod gauge, ordinarily in use in the factory, was unsatisfactory for research purposes, because it does not measure the pressure of condensible gases such as the vapours of water, oil or mercury. Sir Joseph Thomson very kindly considered the question and suggested that attention should be directed to the Knudsen Manometer, which is free from this defect.

From the Kinetic Theory of Gases, Knudsen ("Ann. der Physik," XXXII., p. 812) deduced that the pressure of gas in equilibrium in a closed apparatus varies from point to point as the square root of the absolute temperature of the gas, when the dimensions of the apparatus are very small compared with the mean free path of the molecules. This proposition will be referred to as . . . . (1)

Smoluchowski ("Ann. der Physik.," XXXIV., 182) shows that Knudsen's formula requires that the moving plate should be close to fixed plates on *both* sides.

Without going into the general theory, we may notice the peculiarities of some typical cases. Consider a gas confined between two indefinitely large plane parallel plates at uniform but different temperatures, and suppose the gas so rarefied that collisions occur with the plates only. The motion of each molecule is a series of journeys in straight lines between the plates. On each journey the molecule gives out on stopping exactly the translatory momentum in a direction normal to the plates which it received on starting. So the pressures on the two plates must be equal, however widely their temperatures may differ. At first sight this might appear to contradict Knudsen's statement (1), but it does really not do so, for it will be shown that the temperature of the gas is perfectly uniform. The kinetic energy of a particle which last collided with the hot solid is presumably greater on the average than that of one coming from the cold solid. But the temperature, measured by the mean kinetic energy, depends on the relative numbers of these two classes of molecules in the element of volume. Denote this ratio by G. From the symmetry it

follows that G is uniform throughout the gas, and so the temperature is also uniform. The only way in which we can set up an inequality of temperature in the gas is by disturbing the symmetry, as, for instance, by confining the hotter area on the plates to a small patch on one of them. In the neighbourhood of the patch, G will differ from its value in the remote parts of the region.

In the present instrument the temperature is nearly uniform over each plate, but the symmetry is disturbed by the insertion between them of a small disc, which may be said to separate off a pair of compartments one on either side of it, communicating round its edge. Suppose, for simplicity, that the disc is midway between the fixed plates and parallel to them. The rates at which molecules are shot into the two compartments from the distant parts of the apparatus must be approximately equal. To the same approximation in the steady state the particles must be shot out of the two compartments at equal rates. But the rate of loss from either compartment must be proportional to the density and to the mean velocity in that compartment, as in the effusion experiments of Graham and Osborne Reynolds (Jeans, "Dynamical Theory of Gases," 2nd edition, § § 168, 170). Hence, when we compare the compartments with one another, the square roots of the absolute temperatures vary directly as the densities, and by Boyle's law inversely as the pressures, as in (1) above.

Knudsen constructed an instrument operated by these local differences of pressure. However, the instrument, as originally described by him, could only be kept in operation for a few minutes at a time, as it depended on an inequality of temperature which obliterated itself soon after it had been set up.

The following instrument was designed in consultation with Mr. R. M. Abraham, of Messrs. C. F. Casella & Co. It was made by that firm in 1912 and 1913.

The hot and cold plates were of glass and about 10 cm. in diameter. They were separated by 0·54 cm. by a glass ring. One of the plates had a hole drilled in it for the insertion of the connection to the vacuum pump. The joints were made tight by the special soft sealing wax in use at Owen's College, Manchester.

The flat glass box, so formed, was placed between two massive copper slabs each 1·15 cm. thick.

A difference of temperature of about 10°C. could be maintained indefinitely between the glass plates by warming one of

the copper slabs by means of a small gas flame, or by cooling it with ice. To eliminate electrostatic forces the inner faces of the glass plates were platinized and were put in electric connection by a wire spring which pressed against both.

The moving system is shown in Fig. 1. Inside the glass box a disc of mica of 3·5 cm. diameter was free to approach or recede from the hotter glass plate by rotation about an axis in the plane of the mica, but some distance beyond its edge. This axis was formed from two pieces of tungsten wire, one above and one below in the same straight line.

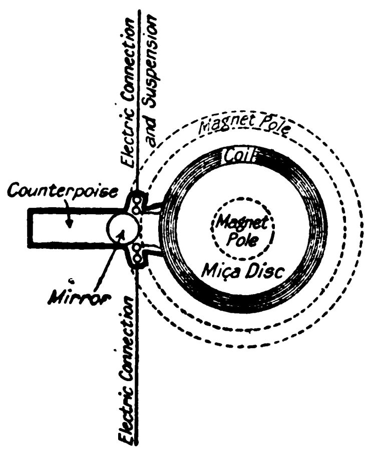


FIG. 1.

The force of the molecular bombardment tending to drive the moving disc away from the hotter glass plate, was balanced by an electromagnetic force. This force was produced by an electric current which flowed in a coil round the edge of the moving disc, and which was acted upon by a magnetic field directed along the radii of the circular coil. The poles of the magnet are shown in Fig. 1; they lay one on either side of the glass box. The object of this device was that the electromagnetic effect on the suspended system should be, not the usual couple, but a single force acting at the centre of the moving disc and at right angles to it.

The angular position of the moving system was observed by the light reflected from a mirror which it carried. The angle was brought exactly to zero by the current. The mica vane was kept approximately midway between the glass plates, but

its exact position in this translatory sense could not be observed, and, on Knudsen's theory, did not matter.

A pair of thermo-junctions of copper-eureka were nearly in contact with the outer sides of the glass box opposite the centre of area of the moving system. The E.M.F. given by this circuit was balanced on a potentiometer wire, through which flowed the current in the moving coil.

A defect of the instrument as constructed was a considerable twist in the suspension wire, which required a current J_0 to balance it, when both plates were at the same temperature. When a temperature-difference had been established a different current J_r was required. Thus, $J_r - J_0$ was proportional to the mechanical force produced by the gas. If we denote by r the resistance of the potentiometer wire required to make a balance with the thermo-junctions, then $r \,.\, J_r$ was proportional to the difference of temperature $(T_1 - T_2)$ between the affixed plates. So that the mechanical force per temperature difference was proportional to

$$\frac{1}{r}\left(1-\frac{J_0}{J_r}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Next, Knudsen's theory can be adapted to present circumstances as follows: The mechanical force per area is the difference of the gas pressures P_1 and P_2 on the two sides of the moving disc. The temperatures of the gas on the two sides may be taken to be $M+\frac{1}{4}\Delta$ and $M-\frac{1}{4}\Delta$, where M is the mean of T_1 and T_2 and Δ is their difference $T_1 - T_2$.

Now, if the gas in the instrument is in communication with a McLeod gauge which has a temperature T_3 and which registers a pressure P_3, then from Knudsen's formula (1) we shall have for the pressures P_1 and P_2 on the two sides of the moving disc:

$$\frac{P_1}{P_3}=\left\{\frac{M+\frac{1}{4}\Delta}{T_3}\right\}^{\frac{1}{2}}; \quad \frac{P_2}{P_3}=\left\{\frac{M-\frac{1}{4}\Delta}{T_3}\right\}^{\frac{1}{2}}, \quad . \quad . \quad . \quad (3)$$

whence, on expanding by the Binomial theorem,

$$\frac{P_1-P_2}{P_3}=\frac{\Delta}{4\sqrt{MT_3}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3a)$$

if terms in Δ^3 and higher odd powers be neglected.

So $$\frac{4\sqrt{MT_3}\,.\,(P_1-P_2)}{\Delta}=P_3. \quad . \quad . \quad . \quad . \quad (4)$$

Now, $(P_1-P_2)/\Delta$ is the mechanical force per area per difference of temperature, and hence by (2)—

$$C\frac{\sqrt{MT_3}}{r}\left(1-\frac{J_0}{J_r}\right)=P_3, \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

where C is a permanent instrumental constant to be determined by experiment. Note that this formula appears to assume that the McLeod gauge and the connecting tube were both small compared with the mean free path. That condition was not satisfied. If the initial twist were zero, and if the temperature T_3 of the McLeod gauge were equal to the mean temperature M of the Knudsen apparatus, then this formula would take the very simple form,

$$\frac{P}{T}=\frac{C}{r} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (6)$$

where T is this temperature; that is to say, the *number of molecules per unit volume would be inversely proportional to the resistance of the potentiometer wire.*

Facilities for testing the apparatus were kindly afforded by Sir Ernest Rutherford at Owen's College.

The apparatus was exhausted by an oil pump and then by charcoal in liquid air, and was left in that state overnight. It was then again exhausted by liquid air twice; thus, it is probable that oil and water vapour were thoroughly removed. On the second occasion the connection to the charcoal tube was left open for half an hour after the McLeod gauge had begun to register a pressure less than 0·1 dyne/cm.². At the end of this time a reading of both gauges was taken. Next, the charcoal tube was shut off, so that the pressure gradually rose owing to the small leak in the apparatus. During this stage the other readings were taken. The contents were presumably mercury-vapour and air.

Fig. 2 shows the relation obtained in this way between the pressure P_3 by the McLeod gauge and

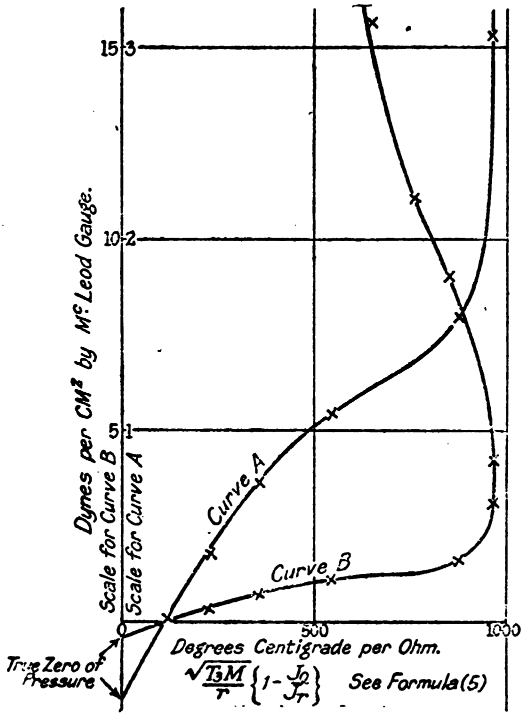
$$\sqrt{T_3M}\,.\,\frac{1}{r}\left(1-\frac{J_0}{J_r}\right)$$

from equation (5). The two curves represent the same function on different scales.

By extrapolation of curve A it is seen that the zero error of the McLeod gauge appears on this occasion to be about 0·4 dyne cm.$^{-2}$, which would correspond to half the maximum

pressure of mercury vapour at the temperature of the McLeod gauge, namely, 0·8 dyne cm.$^{-2}$ at, say, 13°C.

If we then take the true pressure to be given by the addition of 0·4 dyne cm.$^{-2}$ to the pressure obtained from the McLeod gauge, we may say that the useful range of this instrument of Knudsen's type extended up to 2·0 dynes cm.$^{-2}$. The mean temperature of the glass box was about 300°A. On this basis mean free paths have been computed from the data given for



PRESSURE BY KNUDSEN GAUGE.

FIG. 2.

air by Jeans (" Dyn. Theory of Gases," 2nd ed., p. 341). The top of the useful range was reached at 2·0 dynes cm.$^{-2}$ when the mean free path was six times the distance of 0·54 cm. between the inner faces of the glass box.

Curve B, drawn on a reduced scale, shows that the mechanical force per temperature-difference attained a maximum when the corrected pressure was 4·0 dynes cm.$^{-2}$, so that the

mean free path was 3·0 times the distance between the fixed plates. The maximum is, of course, outside the range of the formulæ (1) to (6), which only apply to the initial linear part.

The neglected terms in (3*a*) do not amount to 1/5,000 of the term which is retained, when $\Delta=17°C.$, as it was during the test, so that they cannot account for the curvature of the graph.

The chief difficulty experienced in use was that the electro magnetic force affected the period of oscillation of the coil, and in some circumstances rendered it unstable. Instability could probably be got rid of by new pole-pieces for the magnet, symmetrically designed to give as uniform a field as possible.

The vacuum-tightness of the apparatus may be measured inversely as a leak expressed as the rate of rise of pressure multiplied by the volume of all the connected vacuous cavities. It was found to be 0·2 dyne cm.$^{-2}$, sec.$^{-1}$, cm.^{3}. This would comprise also any evolution of occluded gases.

The resistance of the moving coil and its leading-in wires was 50 to 60 ohms. The current through it to balance the maximum force of the gas was about 1/700 ampere-hour per 10°C. difference between the plates, so that the electric heating of the coil was negligible.

Work upon apparatus had to be abandoned in 1913 owing to the pressure of other operations. Since then a vacuum-manometer depending on viscosity has been brought out by Dr. P. E. Shaw, and investigated theoretically by Mr. F. J. W. Whipple. A Paper on the Knudsen manometer, by J. W. Woodrow, has appeared in the " Physical Review " for December, 1914.

Summary.

The McLeod gauge has a false zero of pressure if condensible vapours are present. The Knudsen instrument is free from this defect, but has a very limited range. It operates on the principle that when the molecules collide only with the solid parts of the apparatus, then they knock a free vane away from a hotter towards a colder surface. The instrument as originally described by Knudsen could only be kept in operation for a few minutes at a time. The action of the present instrument could be maintained indefinitely. Its range extends up to 2·0 dynes cm.$^{-2}$. The force of the molecular bombardment is balanced by the effect of a magnetic field of special form, acting upon an electric current attached to the vane, and

the temperature difference is measured by a thermo-junction, the E.M.F. of which is balanced against the same current in a potentiometer. The instrument was constructed by Messrs. C. F. Casella & Co.

DISCUSSION.

Mr. R. S. Whipple said he was not quite clear as to how the plate moved.

Mr. C. C. Paterson asked if the size of the tube entering the manometer had much effect on the results.

Dr. D. Owen asked if it would not be better to measure the temperature difference between the glass plates instead of the copper plates outside. As it was, the instrument had to be calibrated empirically, and could not be used to give a check on Knudsen's formulæ. With regard to curve 2, the author said that the mechanical force per unit temperature difference was a maximum at 4 dynes per square centimetre, and then stated that this lies outside the range of the formulæ. What, then, was the meaning of this part of the curve? What was the lowest pressure which the author was able to measure? If J_0' was nearly equal to J_r, the method evidently became inaccurate. Could not J_0 be eliminated?

The Author, in reply, said that the plate moved at right angles to its own plane. The length of leading-in tube was about a metre, and its diameter about 1 cm. He did not remember the precise figures. The difference of temperature of the glass plates could easily be measured directly; but it was easy to show that, under steady conditions, the temperature of the copper plate was a very fair measure of that of the glass. As regards a check on Knudsen's theory, there was at least a partial check, inasmuch as the first parts of the curve were straight. The linear law was not assumed, otherwise the diagram would consist of two straight lines. The lowest pressure he had reached was 0·4 dyne per square centimetre, owing to the vapour pressure of mercury in the apparatus. It would be useful to eliminate J_0 by taking extra trouble with the suspension wire; but it did not introduce inaccuracy, as it simply had to be measured.

XX. *On Theories of Thermal Transpiration.* By Gilbert D. West, *M.Sc* (*Lond.*).

Received May 8, 1919.

In connection with an experimental research arising out of measurements of the pressure of light,* the author has had occasion to consider the various theories that have been advanced to explain the phenomena of thermal transpiration. Of those considered, that formulated by Sutherland in 1896† has been most helpful. It is the object of the present Paper

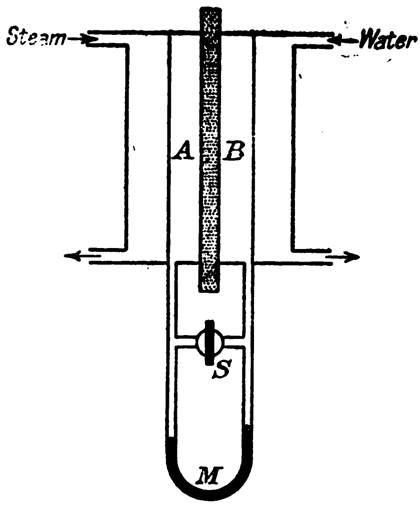


Fig. 1.

to indicate the relation of this theory to other theories, to show how it is capable of explaining work already done, and to put it into a form that will be of use in the further research it is hoped shortly to publish.

It is to Osborne Reynolds that we owe the discovery of thermal transpiration.‡ Reynolds used an apparatus somewhat similar to that shown in Fig. 1. The apparatus consists essentially of two chambers *A* and *B*, separated by a plate of porous material such as meerschaum. Means are provided for

* "Proc." Phys. Soc., XXVIII., p. 259, 1916.
† "Phil. Mag.," XLII., p. 373, 1896.
‡ Phil. "Trans.," CLXX., p. 727, 1879.

maintaining the chambers at different temperatures. M is a mercury manometer, and S is a stopcock.

On opening the latter, the pressures in A and B are equalised. When, however, it is closed again, the pressure in A gradually rises, as the result of the passage of gas from B to A through the meerschaum. When the mean free path of the gas molecules is large compared with the size of the pores, it is found that the final pressures in the chambers A and B are proportional to the square root of their absolute temperatures, T_A and T_B. It should be noted that the nature of the gas is unimportant.

The usual explanation of this result is somewhat as follows. Suppose the porous plate replaced by a non-conducting lamina with a single perforation, small compared to the mean free path of the gas molecules. Let N_A and V_A represent respectively the number and root-mean-square velocity of the molecules in the compartment A, and likewise let N_B and V_B represent similar magnitudes in regard to the compartment B. If all the molecules be divided into Joule's six conventional sets, moving parallel and perpendicular to the lamina, we shall have $\frac{1}{6}N_A V_A$ and $\frac{1}{6}N_B V_B$ impacts per square centimetre respectively on each side of the lamina. When equilibrium is reached the numbers of molecules passing each way through the orifice must be the same, and hence

$$\tfrac{1}{6}N_A V_A = \tfrac{1}{6}N_B V_B.$$

Thus, if m be the mass of a molecule, the ratio of the pressure in A and B is given by

$$\frac{P_A}{P_B} = \frac{\frac{1}{3}N_A m V_A^2}{\frac{1}{3}N_B m V_B^2} = \frac{V_A}{V_B} = \sqrt{\frac{T_A}{T_B}}.$$

There is no difficulty in extending this calculation to the case where the orifice is replaced by a fine bore tube, along which a temperature gradient is maintained, and the step to a porous plate is then simple.

A straightforward explanation can thus be given of the experimental results at low pressures. When, however, with rise of pressure, the mean free path of the molecules becomes comparable to, or less than the size of the pores, difficulties arise, and the simple theory previously given, no longer holds.

An elaborate investigation of the phenomena at such pressures was given by Osborne Reynolds, but it was presented in a form so abstruse that it is doubtful to what extent it cleared the minds of most physicists. Sutherland, for instance, in

his Paper* remarks that "unfortunately the mathematical form of Reynolds' theory is wearily cumbersome; one gathers that Maxwell found it distasteful, and Fitzgerald ("Phil. Mag.," (5) XI.) describes it as inelegant and unnecessarily elaborate . . . but what appears to me to be the fatal objection to Reynolds' mathematical method, is that it takes the mind away from definite physical concepts of the actual operation of the causes of thermal transpiration and radiometer motion." He goes on to say that the object of his own Paper "is to construct a theory . . . that will fall into line with the current kinetic theory of gases, and keep the physics of the phenomena to the fore."

Sutherland's Paper came, however, at a time when interest in thermal transpiration and cognate phenomena had died down, and it thus escaped sufficient attention. Sir Joseph Larmor in his article on "Radiometer" in the "Encyclopædia Britannica," does not even mention the Paper, and the Danish physicist, Knudsen, in a Paper† on thermal transpiration in its relation to the equilibrium conditions in a gas, does not mention the Paper, and moreover goes over a certain amount of ground that Sutherland had previously traversed.

From the point of view of the author's further research, both Sutherland's and Knudsen's Papers are of considerable importance, and, although both theories have a somewhat similar basis, the methods of development are so different that it has been thought desirable to show how, by a simple, though perhaps rather approximate, calculation based chiefly on Sutherland's methods, we can arrive at and extend Knudsen's results. More especially is this desirable, as the latter are supported by a number of carefully planned experiments.

Consider (as does Knudsen) the case of a tube along which a temperature gradient dT/dx is maintained. It is necessary to suppose this gradient to be small as otherwise λ the mean free path of the molecules may vary somewhat from point to point, and the caclulation then becomes very difficult. It is further necessary to suppose that the gradient is constant over a distance several times λ, that the diameter of the tube is large compared to λ, and, lastly, that any gas currents that occur in the tube are small in magnitude, in order that the temperature may be considered constant over any cross-section.

* *Loc. cit.*, p. 374.
† "Ann. d. Phys.," XXXI., 1, pp. 205-229, 1910.

Let the tube PQ (Fig. 2) connect two infinite spaces, and fix attention on a small area ds of a cross-section AB. Unless ds is very near the wall of the tube, molecules can be supposed to arrive at this area from points on the surface of a sphere of radius λ, and they can be divided into two classes—those with a component velocity parallel to the axis of the tube and towards the left, and those with a similar component velocity towards the right. If all the molecules in any section be supposed to have the arithmetic mean velocity Ω, and if further, the angle θ (measured according to the ordinary convention) represent

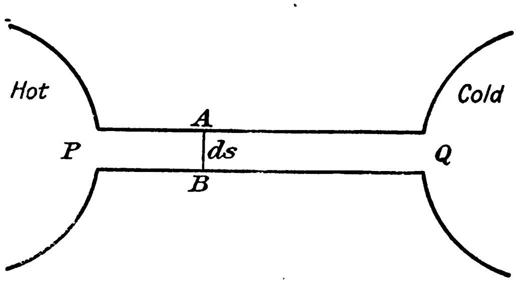


FIG. 2.

the inclination of the direction of motion of any molecule to the axis of the tube, the velocity of this molecule will be $\Omega+\frac{d\Omega}{dx}\lambda\cos\theta$, whether it comes from the left or right. Hence, if from a point O we draw lines representing the velocities of all the molecules that arrive at ds, from, say, the left, we shall have a diagram similar to Fig. 3, in which

$$OR=\Omega+\frac{d\Omega}{dx}\lambda, \text{ and } OS=\Omega.$$

Calculation is now much simplified if we assume all the molecules to have the same velocity $\Omega-\frac{1}{2}\frac{d\Omega}{dx}\lambda$. This approximation substitutes a hemisphere of radius $\Omega-\frac{1}{2}\frac{d\Omega}{dx}\lambda$ for the previous curved surface, and is hence more justifiable the smaller the value of $d\Omega/dx$. Likewise, we shall assume that if N be the number of molecules per unit volume at ds, the

number of molecules per unit volume possessing the velocity

$$\left(\Omega+\tfrac{1}{2}\lambda\frac{d\Omega}{dx}\right) \text{ is } \tfrac{1}{2}\left(N+\tfrac{1}{2}\lambda\frac{dN}{dx}\right).$$

On the basis of these assumptions, it thus appears that on the average the molecules at AB are made up of those arriving from distances $\frac{1}{2}\lambda$ to the left and right of AB respectively. Now it can be shown without difficulty* that the mean component velocity parallel to the axis of the tube of, say, all the molecules from the left possesssing the velocity $\left(\Omega+\frac{1}{2}\frac{d\Omega}{dx}\right)$ is given by

$$\tfrac{1}{2}\left(\Omega+\tfrac{1}{2}\lambda\frac{d\Omega}{dx}\right),$$

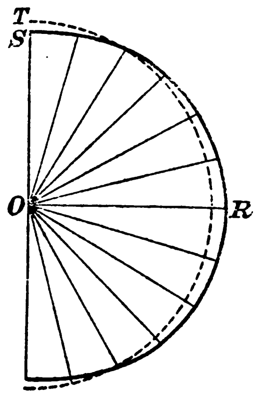


FIG. 3.

and hence the total mass flowing per second through unit area from left to right is

$$\tfrac{1}{4}\left(N+\tfrac{1}{2}\lambda\frac{dN}{dx}\right)\left(\Omega+\tfrac{1}{2}\lambda\frac{d\Omega}{dx}\right)m,$$

where m is the mass of a molecule, or approximately

$$\tfrac{1}{4}m\left\{N\Omega+\tfrac{1}{2}\lambda\Omega\frac{dN}{dx}+\tfrac{1}{2}\lambda N\frac{d\Omega}{dx}\right\},$$

since

$$\tfrac{1}{4}\lambda^2\frac{dN}{dx}\,\frac{d\Omega}{dx}$$

is a second order small quantity, and may be neglected.

* Meyer, "Kinetic Theory of Gases," 2nd ed., p. 83.

Similarly, we could show that the total mass flowing per second through unit area from right to left is given by

$$\tfrac{1}{4}m\left\{N\Omega-\tfrac{1}{2}\lambda\Omega\frac{dN}{dx}-\tfrac{1}{2}\lambda N\frac{d\Omega}{dx}\right\}.$$

Thus, the excess mass flowing from right to left through unit area in unit time is given by

$$-\tfrac{1}{4}m\left\{\lambda\Omega\frac{dN}{dx}+\lambda N\frac{d\Omega}{dx}\right\},$$

or,

$$-\tfrac{1}{4}mN\lambda\Omega\left\{\frac{1}{N}\,\frac{dN}{dx}+\frac{1}{\Omega}\,\frac{d\Omega}{dx}\right\}.$$

We have assumed that all the molecules have the same velocity Ω, but it is more accurate to assume them distributed according to Maxwell's law. In that case we must change $\frac{1}{4}$ to $3\pi/32$, and write the previous expression

$$-\frac{3\pi}{32}mN\lambda\Omega\left\{\frac{1}{N}\,\frac{dN}{dx}+\frac{1}{\Omega}\,\frac{d\Omega}{dx}\right\}.$$

Since, however, p the pressure of a gas, is given by

$$p=\frac{\pi}{8}Nm\Omega^2,$$

it follows that

$$\frac{dp}{dx}=\frac{\pi}{8}m\Omega^2\frac{dN}{dx}+\frac{2\pi}{8}Nm\Omega\frac{d\Omega}{dx},$$

$$=p\left\{\frac{1}{N}\,\frac{dN}{dx}+\frac{2}{\Omega}\,\frac{d\Omega}{dx}\right\}.$$

And further, since according to Maxwell η, the coefficient of viscosity of a gas, is given by $\eta=0{\cdot}31mN\lambda\Omega$, we may write the excess mass moving per second through a tube of radius R as

$$-\pi R^2\frac{3\pi}{32}\,\frac{\eta}{0{\cdot}31}\left\{\frac{1}{p}\,\frac{dp}{dx}-\frac{1}{\Omega}\,\frac{d\Omega}{dx}\right\},$$

or as

$$-2{\cdot}98R^2\eta\left\{\frac{1}{p}\,\frac{dp}{dx}-\frac{1}{2T}\,\frac{dT}{dx}\right\},$$

since the temperature T is proportional to Ω^2.

If the spaces at the ends of the tube are infinite this flow, which is uniform over the whole cross section, will continue as long as the temperature gradient is maintained. If, however, the spaces are limited, the flow will continue only until a sufficient pressure is developed on the hot side to

cause an equal flow of gas in the reverse direction. Such a counter-flow of gas, however, can only take place in conformity with the laws of flow in a capillary tube, whereby the velocity, starting from a maximum along the axis, must decrease progressively until the wall of the tube is reached. The result of the superposition of this flow on the uniform flow in the reverse direction will be to give us a gas current near the surface of the tube from the cold to the hot vessel, and a current in the reverse direction along the axis. Between the two there will be a surface of zero velocity. This superposition of two flows is the basis of Sutherland's method, and Fig. 4 is intended to give an idea of the distribution of velocities in this case.

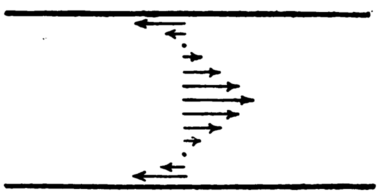


FIG. 4.

The simple formula of Poiseuille gives the mass of gas G discharged from a tube by a small pressure gradient as

$$G = \frac{\pi}{8} \frac{\rho_1 p R^4}{\eta} \frac{dp}{dx},$$

where ρ_1 is the density of the gas at T°C. and under a pressure of 1 dyne per square centimetre. The formula, however, takes no note of the "slip" that occurs at the surface of the tube. This latter is very important at the lower pressures, and hence a more elaborate expression is necessary. Various formulæ have been proposed,* and they differ in points of detail, but the following serves the present purpose—

$$G = \left\{ \frac{\pi}{8} \frac{\rho_1 p}{\eta} R^4 + \frac{4}{3} \sqrt{2\pi\rho_1} R^3 \right\} \frac{dp}{dx}.$$

Equating, therefore, the masses discharged in the reverse directions, we have

$$-\frac{2{\cdot}98 R^2 \eta}{p} \frac{dp}{dx} + \frac{2{\cdot}98\eta}{2T} R^2 \frac{dT}{dx} = \left\{ \frac{\pi}{8} \frac{\rho_1 p}{\eta} R^4 + 3{\cdot}34 \sqrt{\rho_1} R^3 \right\} \frac{dp}{dx}. \quad (1)$$

* Fisher, "Phys. Rev.," XXIX., p. 325, 1909.

Whence

$$\frac{dp}{dT}=\eta^2\Big/\{2\eta^2T/p+37\cdot1R\eta\sqrt{\rho_0T}+65\cdot1\rho_0pR^2\}, \quad . \quad . \quad . \quad (2)$$

where ρ_0 =mass of 1 cubic cm. of gas at 0°C. under 1 dyne/cm.^{2} pressure.

This formula has been deduced on the assumption that the pressure is of a value such that R is large compared to λ, and it will be remembered that we found the molecules could be considered to come from distances $\frac{1}{2}\lambda$ from either side of the section AB in Fig. 2. When the pressure is so low that λ is large compared to the diameter of the tube, this is not so. We may replace the $\frac{1}{2}$, however, by a quantity k, which will be very approximately constant, so long as λ is great.

Hence, instead of equation (1), we must now write

$$-\pi R^2\frac{3\pi}{32}mN\lambda\Omega\left\{\frac{2k}{p}\frac{dp}{dx}-\frac{2k}{T}\frac{dT}{dx}\right\}$$

$$=\left\{\frac{\pi}{8}\frac{\rho_1p}{\eta}R^4+\frac{4}{3}\sqrt{2\pi\rho_1}R^3\right\}\frac{dp}{dx}.$$

Since, however, λ is by supposition much greater than R, the equation reduces in the limit to an equation independent of k, namely,

$$\left\{\frac{1}{p}\frac{dp}{dx}-\frac{1}{2T}\frac{dT}{dx}\right\}=0,^*$$

which gives us $$\frac{dp}{dT}=\frac{p}{2T}.$$

Now, this is the limit to which the previous formula (2) reduces when p is made very small. Hence, the formula 2 applies to both high and low pressures. In regard to medium pressures, we should in strictness have to introduce an appropriate value of k, but it appears that no great error is made by leaving the formula untouched. Hence, the equation

$$\frac{dp}{dT}=\eta^2\Big/\left\{\frac{2\eta^2T}{p}+37\cdot1R\eta\sqrt{\rho_0T}+65\cdot1\rho_0pR^2\right\}$$

may be said to apply approximately to all pressures. The variation of dp/dT with gas pressure for a tube of definite radius is shown in the accompanying curves. It will be seen

* Physically, this implies that, in the condition of equilibrium there is no flow—an assumption made in the simple explanation given at the beginning of the Paper.

that with decreasing pressure dp/dT rises, reaches a maximum and then falls.

It is now necessary to compare these results with those of Knudsen. An outline of his method is somewhat as follows :

A calculation is first made of the traction a small length of tube, along which a temperature gradient is maintained, would experience in the absence of gas currents. In a detailed calculation it is then shown how the traction is modified when the peripheral gas current from the cold vessel discharges as much as the axial current from the hot vessel. He equates the traction thus found to that calculated from the pressure gradient and the cross-section of the tube.

As a result he finds for high pressures, and for medium pressures approached from the high-pressure side,

$$\frac{dp}{dT}=\eta^2K\Big/\{43{\cdot}5R\eta\sqrt{\rho_0 T}+68{\cdot}4\rho_0 R^2p\},$$

where K varies from 1 at high pressures to 4/3 at very low pressures. Likewise, for low pressures and for medium pressures approached from the low-pressure side, he finds that

$$\frac{dp}{dT}=\tfrac{3}{8}Kp\Big/\left(1+\frac{2R}{\lambda}\right)T,$$

which, when $K=4/3$ can be reduced to

$$\frac{dp}{dT}=p\eta\Big/\{2T\eta+32{\cdot}7pR\sqrt{\rho_0 T}\}.$$

When $K=1$, 32·7 is replaced by 43·5—*i.e.*, the value in the previous formula.

Except in so far as Knudsen's results are not combined in a single formula, they are of the same form as those obtained in this work, whilst the discrepancy of the coefficients may be explained chiefly by the variation of K. The principles laid down by Sutherland would hence appear to form an adequate basis for the more modern work of Knudsen.

The accompanying curves plotted on semi-logarithmic paper so as to secure a greater range, illustrate well the relationship of the various formulæ. At low pressures we see that both agree in making dp/dT, the pressure difference per degree difference of temperature, proportional to the pressure, and independent of the nature of the gas. Further, at high pressures dp/dT becomes inversely proportional to the pressure and dependent on the nature of the gas. Both these

results are in accord with the experiments of Osborne Reynolds, to which previous reference has been made.

For the purposes of the experimental verification of his theory Knudsen found it necessary to integrate some of his equations, in order that they might apply when large differences of temperature existed. His results calculated in this way were in good accord with those obtained by his experiments, and considerable confidence may therefore be felt in

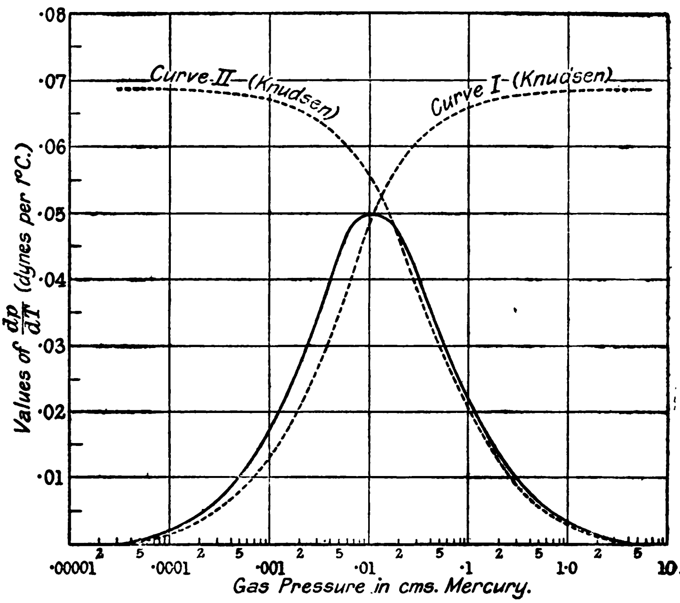


FIG. 5.—THERMAL TRANSPIRATION FORMULÆ FOR A CYLINDRICAL TUBE OF RADIUS 1 CM.

Full line curve given by $\dfrac{dp}{dT}=\eta^2\Big/\left\{\dfrac{2\eta^2T}{p}+37{\cdot}1R\eta\sqrt{\rho_0T}+65{\cdot}\rho_0pR^2\right\}$.

Dotted line curve I. given by $\dfrac{dp}{dT}=\frac{3}{8}p\Big/\left(1+\dfrac{2R}{\lambda}\right)T$ (Knudsen).

Dotted line curve II. given by $\dfrac{dp}{dT}=\eta^2\Big/\left\{43{\cdot}5R\eta\sqrt{\rho_0T}+6{\cdot}84\rho_0R^2p\right\}$ (Knudsen)

them. It is hence satisfactory to find that those developed in this Paper do not differ very materially from those of Knudsen, and for approximate calculations they are thus quite adequate.

Summarising, therefore, we may say that if two vessels containing gas at different temperatures are connected by a

capillary tube, the phenomena vary according to the relation of the mean free path of the molecules to the radius of the tube.

At very low pressures, gas flows from the cold vessel to the hot vessel until a sufficient pressure is developed to check it. If the difference in temperature be small, the pressure difference is proportional to the gas pressure in the two vessels, is independent of the nature of the gas, and is given in fact by

$$dp=\frac{p}{2T}dT.$$

With increasing pressure, the pressure difference rises less rapidly, and eventually reaches a maximum. It then begins to fall off and finally diminishes inversely as the pressure, approximately according to the equation,

$$dp=\eta^2 dT/65{\cdot}1\rho_0 pR^2.$$

In these latter stages a circulation of gas is maintained, and currents flow from cold to hot along the surface of the tube, and from hot to cold along the axis.

Summary.

The process of thermal transpiration, or the establishment of a pressure difference between two vessels connected by a capillary tube and at different temperatures, takes place at all gas pressures. The explanation of the phenomena at low pressures is well known and simple. When, however, the mean free path of the molecules is of the order of, or smaller than, the diameter of the tube, the simple explanation fails and a more elaborate hypothesis is necessary. Reference is made in the Paper to the work of Reynolds, Sutherland and Knudsen. The two latter investigators have proceeded on different lines, but it is shown how Sutherland's original method of treatment can be employed to calculate a formula which is applicable to all pressures, and which is in approximate agreement with the formulæ given by Knudsen for limited ranges of pressure. It is anticipated that the present Paper will be of considerable use in the author's further research.

DISCUSSION.

Mr. F. J. WHIPPLE observed that the author had adopted the simplification of assuming all the molecules to have the mean velocity. Had he worked out the full treatment on the basis of the Maxwellian distribution of velocities? He stated that in order to bring the formula into accord with the Maxwellian distribution 1/4 had to be replaced by $3\pi/32$. Was it

easy to show this? He had not followed the author's explanation of the circulation set up in the tube. It was easy to see why the pressure-difference flow should be faster near the axis of the tube than at the walls; but the assumption that the temperature-difference flow should be uniform across the tube seemed somewhat arbitrary.

Dr. H. S. ALLEN was glad to hear the author's tribute to Sutherland. He had come across many cases of neglect of Sutherland's work. For example, the relation between the coefficient of expansion and the specific heat of an element usually attributed to Grüneisen, was first published by Sutherland, and there were many similar cases which could be quoted.

Prof. LEES congratulated the author on the simple way in which he had arrived at Knudsen's results.

The AUTHOR, in reply to Mr. WHIPPLE, said the numerical transformation referred to was simply made in conformity with the prevalent custom of replacing 1/4 by $3\pi/32$ in such calculations. As regards the distribution of the temperature-difference flow, the value near the centre is obtained by integration over a hemisphere, while near the edges integration had to be performed over a smaller area—say half a hemisphere. The values obtained did not differ greatly at different parts of the cross-section.

XXI. *A Comparison of the Wave-form of the Telephone Current Produced by a Thermal Detector and by a Rectifier in Heterodyne Reception.* By BALTH. VAN DER POL, Jun., D.Sc. (*Utrecht*).

COMMUNICATED BY PROF. W. H. ECCLES.

RECEIVED MAY 9, 1919.

THE heterodyne method of receiving wireless signals is principally used for continuous wave reception. The well-known procedure is to superimpose on the sinusoidal antenna current due to the waves, another sinusoidal current generated locally. The wave-form of this complex current is then modified by some wave distorting detector in such a way that at least one harmonic will fall within the region of audibility, thus producing a sound in the telephone receiver.

The question naturally arises of what wave-form the resulting current will be.

A theoretical treatment obviously depends on the assumptions made underlying the action of the detector.

When a detector is used generating a potential difference at the telephone terminals at any time proportional to the square of the total antenna current the theory is very simple indeed. For, if $I_1 \cos \omega_1 t$ is the current due to the incoming waves and $-I_2 \cos \omega_2 t$ is the current generated locally, the potential difference at the terminals of the telephone receiver is obviously proportional to

$$(I_1 \cos \omega_1 t - I_2 \cos \omega_2 t)^2 = \tfrac{1}{2}(I_1{}^2 + I_2{}^2) + \tfrac{1}{2}(I_1{}^2 \cos 2\omega_1 t + I_2{}^2 \cos 2\omega_2 t) - I_1 I_2 \{\cos(\omega_1 + \omega_2)t + \cos(\omega_1 - \omega_2)t\}.$$

and the sinusoid of lowest frequency is seen to be

$$-I_1 I_2 \cos(\omega_1 - \omega_2)t.$$

If, on the other hand, a rectifier is used, *i.e.*, a system having a finite constant resistance for currents in one direction and an infinite resistance for currents in the other direction, the theory, if no approximations are to be used, becomes more involved. The telephone current in this case will be

$$\left.\begin{array}{c} I_1 \cos \omega_1 t - I_2 \cos \omega_2 t, \\ \text{with the condition that, when this func-} \\ \text{tion is negative, it is replaced by zero.} \end{array}\right\} \quad . \quad . \quad . \quad . \quad (1)$$

The function (1) has zeros of a very complicated nature, and the Fourier analysis, which obviously depends to a large

extent on the values of these zeros, is in general a matter of great difficulty.

If, however, the amplitudes of both sinusoids are taken to be equal (equal heterodyne), the analysis becomes much simpler, and this case only will be considered here. Hence the Fourier analysis is required of

$$\left.\begin{array}{c}\cos\omega_1 t-\cos\omega_2 t,\\ \text{with the condition that, when this func-}\\ \text{tion is negative, it is replaced by zero.}\end{array}\right\} \quad . \quad . \quad . \quad (1a)$$

At the outset it must be remarked that (1a) is only capable of being analysed in a Fourier series when ω_1 and ω_2 are commensurable.

Now, any function $f(t)$, subject to the condition that when it is negative it is replaced by zero, can be represented as

$$\tfrac{1}{2}\{|f(t)|+f(t)\}.$$

The function under consideration is therefore

$$\tfrac{1}{2}\{|\cos\omega_1 t-\cos\omega_2 t|+(\cos\omega_1 t-\cos\omega_2 t)\},$$

which we wish to express in the form

$$\begin{aligned}b_0+b_1\cos ft+b_2\cos 2ft+\ .\ .\ .\\ +a_1\sin ft+a_2\sin 2ft+\ .\ .\ .\ .\end{aligned}$$

(where the meaning of f will be considered later).

The second term $\frac{1}{2}(\cos\omega_1 t-\cos\omega_2 t)$ only contributes to the value of b_{n1} and b_{n2}, where

$$n_1 f=\omega_1 \quad \text{and} \quad n_2 f=\omega_2,$$

and can, therefore, for the present be omitted.

The remaining part

$$\tfrac{1}{2}|\cos\omega_1 t-\cos\omega_2 t|$$

is symmetrical with respect to $t=0$, therefore

$$a_1=a_2=.\ .\ .\ .\ .=0.$$

It can further be expressed as the product of two moduli, viz. :—

$$\begin{aligned}\tfrac{1}{2}|\cos\omega_1 t-\cos\omega_2 t| &=\left|-\sin\frac{\omega_1+\omega_2}{2}t\ .\ \sin\frac{\omega_1-\omega_2}{2}t\right|\\ &=\left|\sin\frac{\omega_1+\omega_2}{2}t\right|\ .\ \left|\sin\frac{\omega_1-\omega_2}{2}t\right|.\quad .\quad .\quad .\quad (2)\end{aligned}$$

Now the Fourier analysis of the modulus of $\sin pt$ is easily found* to be

$$\left.\begin{aligned}|\sin pt| &= \frac{2}{\pi}\left\{1-2\sum_{l=1,2,3\,\ldots}^{\infty}\frac{1}{4l^2-1}\cos 2plt\right\}.\\ &\text{In the same way we have}\\ |\cos pt| &= \frac{2}{\pi}\left\{1-2\sum_{l=1,2,3\,\ldots}^{\infty}\frac{(-1)^l}{4l^2-1}\cos 2plt\right\}.\end{aligned}\right\}\dagger \qquad (3)$$

The function $\frac{1}{2}|\cos\omega_1 t-\cos\omega_2 t|$ can therefore be written

$$\frac{4}{\pi^2}\left\{1-2\sum_{l=1,2,3\,\ldots}\frac{1}{4l^2-1}\cos(\omega_1+\omega_2)lt\right\}\left\{1-2\sum_{l'=1,2,3\,\ldots}\frac{1}{4l'^2-1}\cos(\omega_1-\omega_2)l't\right\}$$

$$\begin{aligned}=\frac{4}{\pi^2}\Bigg\{1&-2\sum_{l=1,2,3\,\ldots}\frac{1}{4l^2-1}\cos(\omega_1+\omega_2)lt,\\ &-2\sum_{l'=1,2,3\,\ldots}\frac{1}{4l'^2-1}\cos(\omega_1-\omega_2)l't,\\ &+4\sum_{l'=1,2,3\,\ldots}\sum_{l'=1,2,3\,\ldots}\frac{1}{4l^2-1}\,\frac{1}{4l'^2-1}\cos(\omega_1+\omega_2)lt\,.\cos(\omega_1-\omega_2)l't\Bigg\}.\end{aligned}$$

Each term of the last double series can be modified into

$$\frac{1}{2}\,\frac{1}{4l^2-1}\,\frac{1}{4l'^2-1}$$

$$[\cos\{(\omega_1+\omega_2)l+(\omega_1-\omega_2)l'\}t+\cos\{(\omega_1+\omega_2)l-(\omega_1-\omega_2)l'\}t],$$

so that we arrive at

$$\left.\begin{aligned}&\tfrac{1}{2}|\cos\omega_1 t-\cos\omega_2 t|=\\ &\frac{4}{\pi^2}\Bigg\{1-2\sum_{l=1,2,3\,\ldots}\frac{1}{4l^2-1}\cos(\omega_1+\omega_2)lt\\ &\quad-2\sum_{l'=1,2,3\,\ldots}\frac{1}{4l'^2-1}\cos(\omega_1-\omega_2)lt\\ &\quad+2\sum_{l=1,2,3\,\ldots}\sum_{l'=1,2,3\,\ldots}\frac{1}{4l^2-1}\,\frac{1}{4l'^2-1}[\cos\{(\omega_1+\omega_2)l\\ &\qquad+(\omega_1-\omega_2)l'\}t+\cos\{(\omega_1+\omega_2)l-(\omega_1-\omega_2)l'\}t]\Bigg\}\end{aligned}\right\} \qquad (4)$$

* See *e.g.* Riemann-Weber. Partielle Diff. Gleichungen I.

† These expressions at once give the analysis for the (*rectified* $\sin pt$)

$$=\tfrac{1}{2}\{|\sin pt|+\sin pt\}=\frac{1}{\pi}\left\{1-2\sum_l\frac{1}{4l^2-1}\cos 2plt+\frac{\pi}{2}\sin pt\right\},$$

and (*rectified* $\cos pt$)

$$=\tfrac{1}{2}\{|\cos pt|+\cos pt\}=\frac{1}{\pi}\left\{1-2\sum_l\frac{(-1)^l}{4l^2-1}\cos 2plt+\frac{\pi}{2}\cos pt\right\}.$$

The fundamental frequency of this function is $\frac{1}{2\pi}$ *times the greatest common factor (including fractions) of* $(\omega_1+\omega_2)$ *and* $(\omega_1-\omega_2)$, *so that we can write*

$$\omega_1+\omega_2=(p+q)f,$$
$$\omega_1-\omega_2=(p-q)f,$$

where $(p+q)$ *and* $(p-q)$ *are the smallest integers allowing* $(\omega_1+\omega_2)$ *and* $(\omega_1-\omega_2)$ *to be expressed in this form.* It is, further, not strictly necessary that f is at the same time the greatest common factor (in the sense referred to above) of ω_1 and ω_2, neither is it in general equal to $\omega_1-\omega_2$.

It is now only necessary to replace t by t'/f to get the required Fourier analysis

$$\left.\begin{aligned}
&\tfrac{1}{2}|\cos\omega_1 t-\cos\omega_2 t|=\\
&\frac{4}{\pi^2}\Big\{1-2\sum_{l=1,2,3\ldots}\frac{1}{4l^2-1}\cos(p+q)lt'\\
&\qquad-2\sum_{l'=1,2,3\ldots}\frac{1}{4l'^2-1}\cos(p-q)l't'\\
&\qquad+2\sum_{l=1,2,3\ldots}\ \sum_{l'=1,2,3\ldots}\frac{1}{4l^2-1}\,\frac{1}{4l'^2-1}\\
&[\cos\{(p+q)l+(p-q)l'\}t'+\cos\{(p+q)l-(p-q)l'\}t']\Big\}
\end{aligned}\right\} \qquad (5)$$

In order to find the amplitude of a certain harmonic the different terms in the series in (5) have to be carefully considered and collected. For instance, the direct component is made up of the first term together with all terms of which the factor of t' under the cosine signs is zero. They can only occur in the last double series, and those pairs of positive integral values of l and l' must be determined (excluding $l=l'=0$), for which either

$$(p+q)l+(p-q)l'=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

or

$$(p+q)l-(p-q)l'=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

where all variables are integers.

These being found, the corresponding expressions

$$2\cdot\frac{1}{4l^2-1}\cdot\frac{1}{4l'^2-1}$$

have to be calculated and the *exact* amplitude of the continuous component is therefore

$$b_0=\frac{4}{\pi^2}\left[1+2\sum_{l}\sum_{l'}\frac{1}{4l^2-1}\cdot\frac{1}{4l'^2-1}\right],$$

where in the sum those pairs of values of l and l' are to be taken which satisfy either (6) or (7).

Obviously if $p>q$, $p>0$ and $q>0$, (7) only can furnish us with those values.

In the same way the amplitude of the fundamental is given by those terms of the double series for which the coefficient of t' equals unity, *i.e.*, for which

$$(p+q)l+(p-q)l'=\pm 1 \quad . \quad . \quad . \quad . \quad . \quad (8)$$

$$(p+q)l-(p-q)l'=\pm 1 \quad . \quad . \quad . \quad . \quad . \quad (9)$$

After the pairs of positive integral values of l and l' have been determined from the four equations (8) and (9), the amplitude of the fundamental is found to be

$$b_1=\frac{8}{\pi^2}\sum_l\sum_{l'}\frac{1}{4l^2-1}\cdot\frac{1}{4l'^2-1},$$

where again the summation is to be extended over all the pairs of values of l and l' which satisfy (8) and (9).

In general, the third term of (5) (second series), viz.,

$$-2\sum_{l'=1,2,3\ldots}\frac{1}{4l'^2-1}\cos(p-q)l't',$$

will *not* contribute to the fundamental, for it would then be necessary that a positive integer l' exists satisfying

$$(p-q)l'=\pm 1,$$

which is only possible when

$$p-q=1,$$

which is not the general case.

This second series, however, does contribute to the amplitude of the harmonic of order $p-q$. In fact we have

$$b_{p-q}=\frac{4}{\pi^2}\left\{-\frac{2}{3}+2\sum_l\sum_{l'}\frac{1}{4l^2-1}\cdot\frac{1}{4l'^2-1}\right\},$$

where the summation is to be extended over the pairs of positive integer values of l and l' satisfying the four equations

$$(p+q)l+(p-q)l'=\pm(p-q) \quad . \quad . \quad . \quad . \quad (10)$$

$$(p+q)l-(p-q)l'=\pm(p-q) \quad . \quad . \quad . \quad . \quad (11)$$

In general the second series in (5) contributes to the amplitude of the harmonics b_{p-q}, $b_{2(p-q)}$, $b_{3(p-q)}$, ..., &c., to the amounts respectively

$$2 \cdot \frac{1}{4 \cdot 1^2 - 1} = \frac{2}{3},$$

$$2 \cdot \frac{1}{4 \cdot 2^2 - 1} = \frac{2}{15},$$

$$2 \cdot \frac{1}{4 \cdot 3^2 - 1} = \frac{2}{35},$$

. . . . . ., &c.

In the same way the first series, viz.,

$$2 \sum_l \frac{1}{4l^2 - 1} \cos (p+q)lt',$$

will contribute only to the amplitude of the harmonics

$$b_{p+q}, \quad b_{2(p+q)}, \quad b_{3(p+q)}, \ldots \ldots \text{ \&c.},$$

to the amounts respectively again

$$\frac{2}{3}, \ \frac{2}{15}, \ \frac{2}{35}, \ldots \ldots \ldots \text{ \&c.}$$

As $(p-q)$ and $(p+q)$ have, by definition, no common factor it will not be possible for the second series and the third to contribute to one and the same harmonic with the exception of $b_{n(p^2-q^2)}$ where n is an integer. However, in the way shown above, the third (double) series $\Sigma\Sigma$ will contribute to every harmonic, and this contribution has in all cases the form of a rapidly converging series,

$$2 \sum_l \sum_{l'} \frac{1}{4l^2 - 1} \cdot \frac{1}{4l'^2 - 1},$$

where the values of l and l' over which the summation is to be extended have to be found from four Diophantine equations, similar to (8) (9) or (10) (11).

The analysis can further easily be extended to the cases of different initial phases from those considered above.

As regards the relative magnitudes of the amplitudes of the different harmonics these are wholly determined by the values of p and q. If $(p+q)$ is much greater than $(p-q)$, as is usually the case in heterodyne reception, the contributions of the double series to the amplitude of a certain harmonic are usually small in comparison with unity or (if they contribute at all)

with the single term furnished by either the first or second (single) series. We see, therefore, that the harmonics of order

$$(p-q),\ 2(p-q),\ 3(p-q),\ \ldots\ldots\ \&c.$$

and

$$(p+q),\ 2(p+q),\ 3(p+q),\ \ldots\ldots\ \&c.$$

will have much greater amplitudes than the remaining harmonics. Again, in this case b_{p-q} will have a value very near that of b_{p+q},

$$b_{p-q} \sim b_{p+q} \sim \frac{-4}{\pi^2}\cdot\frac{2}{3}.$$

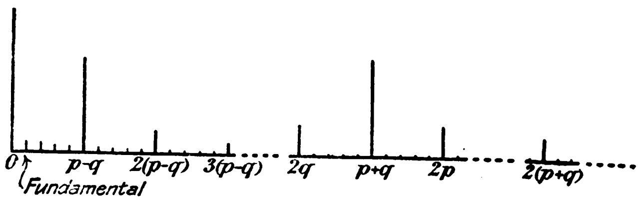
Again

$$b_{2(p-q)} \sim b_{2(p+q)} \sim \frac{-4}{\pi^2}\cdot\frac{2}{15},\ \ldots\ldots\ \&c.$$

If the amplitudes of the different harmonics are represented graphically, we get a figure somewhat like Fig. 1*a*.

We see, therefore, that the amplitude of the harmonic of order $(p-q)$ is for practical cases much bigger than that of the fundamental, and it is likely that the ear will take the

FIG. 1*a*.



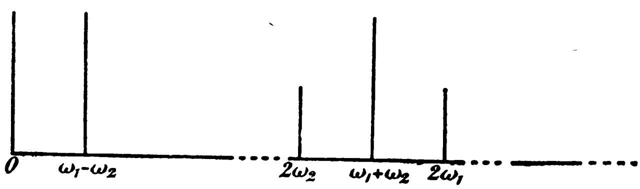


FIG. 1*b*.

harmonic of order $(p-q)$ as determining the pitch of the sound in the telephone receiver. Directly underneath (Fig. 1*b*) we give the amplitudes of the different harmonics that will be present on the assumption of a "thermal" detector, on such a scale that the direct current component in both cases is the same.

From the figure we see further the great theoretical advantage, as far as interference from other waves is concerned, of the thermal detector over the rectifier. While the rectifier gives strong harmonics of the order $2(p-q)$, $3(p-q)$, ..., &c., these fail in the sound produced by the thermal detector. The chance of interference by disturbing waves is, therefore, less when a thermal detector is used than with a rectifier.

In conclusion, we shall compare the mean square current produced by the rectifier (having a constant resistance R in one direction and an infinite resistance in the other direction) with the mean square current obtained with an ordinary ohmic resistance of the same magnitude R in both directions.

In the first case the current will be

$$\tfrac{1}{2}|\cos \omega_1 t-\cos \omega_2 t|+\tfrac{1}{2}(\cos \omega_1 t-\cos \omega_2 t),$$

while in the second case it simply is

$$(\cos \omega_1 t-\cos \omega_2 t).$$

The square of the rectified current is

$$\tfrac{1}{4}\{|\cos \omega_1 t-\cos \omega_2 t|\}^2+\tfrac{1}{4}(\cos \omega_1 t-\cos \omega_2 t)^2+\tfrac{1}{2}|\cos \omega_1 t -\cos \omega_2 t| \,.\, (\cos \omega_1 t-\cos \omega_2 t)=\tfrac{1}{2}(\cos \omega_1 t-\cos \omega_2 t)^2 +\tfrac{1}{2}|\cos \omega_1 t-\cos \omega_2 t| \,.\, (\cos \omega_1 t-\cos \omega_2 t).$$

It follows that if we call I_0^2 the mean square of the unrectified current and I_r^2 the mean square of the rectified current we have

$$I_0^2=1,$$

$$I_r^2=\tfrac{1}{2}+\tfrac{1}{2}\frac{f}{2n}\int_0^{\frac{2n}{f}} (\cos \omega_1 t-\cos \omega_2 t) \,.\, |\cos \omega_1 t-\cos \omega_2 t|\, dt.$$

Now from the above analysis we find at once

$$b_{\omega 1}=\tfrac{1}{2}\frac{f}{2n}\int_0^{\frac{2n}{f}} \cos \omega_1 t \,.\, |\cos \omega_1 t-\cos \omega_2 t|\, dt,$$

and

$$b_{\omega 2}=\tfrac{1}{2}\frac{f}{2n}\int^{\frac{2n}{f}} \cos \omega_2 t \,.\, |\cos \omega_1 t-\cos \omega_2 t|\, dt,$$

where $b_{\omega 1}$ and $b_{\omega 2}$ can be determined in the way shown above.

Hence we finally obtain

$$I_r^2=\tfrac{1}{2}+b_{\omega 1}-b_{\omega 2},$$

as the *exact* value of the mean square of the rectified current.

A reversal of the detector in the circuit would give the mean square current

$$I_r'^2 = \tfrac{1}{2} - b_{\omega 1} + b_{\omega 2}.$$

A *thermal instrument* reading mean square current would theoretically, therefore, give different readings in the two cases.

It follows from the above analysis that a *direct current instrument* would indicate rigorously the same current for both positions of the detector.

As $b_{\omega 1}$ and $b_{\omega 2}$, however, are usually small compared with unity, the readings of the thermal instrument for the two positions of the rectifier will differ very little, and, as was to be expected, we find for the *approximate* value for the mean square rectified current half that of the unrectified current, provided that the unidirectional resistance in the first case is equal to the ordinary resistance in the second.*

* I am indebted to Mr. F. J. W. Whipple for some comments on the unrevised proof.

XXII. *The Magnetic Properties of Varieties of Magnetite.*

By ERNEST WILSON *and* E. F. HERROUN.

RECEIVED MAY 19, 1919.

1. *Introductory.*

MAGNETITE is so widely distributed throughout the earth's crust, and is so interesting in many ways, that no apology is needed for describing experiments which throw further light upon the part played by this wonderful substance in influencing the magnetic properties of the rocks of which it is a constituent, or with which it comes in contact. Much work has already been done :—Abt* examined the retentivity of short and long bars and compared it with that of glass hard steel. Weiss† has made probably the most exhaustive experiments on magnetite crystals. He showed that the substance has a curve of magnetisation resembling that of iron, and that the susceptibility along the different crystallographic axes was different, although tending towards the same limiting value as the force increased. E. Holm ‡ has shown that in the case of Swedish magnetite the susceptibility varies with the magnetising force. The transformation temperature of magnetite has been examined by Barton and Williams, § Curie || and Allan¶.

The primary object of the present communication is to examine the magnetic properties of the various forms in which magnetite is found, ranging from the crystal itself to such substances as magnetite-calcite. Specimens from different parts of the world have been chosen, and the authors have been fortunate in that they have had placed at their disposal the collection of magnetite of the Geological Survey and Museum, numbering about 30 varieties. Prof. H. Louis has supplied certain specimens which he himself has collected, and in addition there are others. The chosen specimens represent widely different characteristics. Variations in magnetic properties as exhibited by crystallised, compact or massive specimens and detached particles, and also the effects of heating have been studied.

* "Wied. Ann." Vol. LII., pp. 749-757, 1894.

† "L'Ecl. electr." Vol. VII., p. 487, 1896. Vol. VIII., p. 56, 105, 1896. "Jour de Phys." 3rd Series, Vol. V., p. 435, 1896.

‡ "Jern-Kontorets Annaler," New Series, 1903, p. 363.

§ B. A. Report, Edinburgh, 1892. "The Electrician," Vol. XXIX., p. 432, 1892.

|| "An de Chim. et de Phys." Vol. V., Series 7, p. 289, 1895.

¶ Physical Society "Journal," Vol XIX., May, 1904.

2. *Instrumentation.*

The type of instrument which has been used for the work described in the present communication, involving as it does the ring method with ballistic galvanometer, was chosen for the following reasons. When the susceptibility is of the order 0·15 or over, the uncertainty of the correction for "end effect" with short specimens becomes serious if the magnetometer is employed. It was not possible to obtain specimens in all cases of greater length than about 4 cm., and an area of cross-section of about 1 sq. cm. was about the practical minimum. An instrument which has formed the subject of a communication to the Royal Society* in connection with recent work carried out for the Geological Survey and Museum begins to have limitations when used to measure susceptibilities of the order 0·15 or over. The instrument actually used is capable of measuring both susceptibilities of a high order and also those so small that the magnetometer can be brought in with accuracy as a check. It has thus been possible to obtain experimental evidence of the accuracy obtainable.

3. *The Ring Magnet.*

The electromagnet which has been used in the experiments for the measurement of susceptibility consists of a ring built up of stampings of stalloy, having an air-gap 2 cm. wide with its sides parallel to a diameter. The specimen is 4 cm. long and has a square section 1 cm. across each side, and is inserted in rectangular holes cut in the pole-pieces of the electromagnet, thus bridging across the air space. Each of the stampings of stalloy has a thickness of 0·32 cm. and they are nine in number. The internal diameter is 7·62 cm. (3 in.) and the external diameter 12·7 cm. (5 in.) The three central stampings, with two additional thin ones to make up the required thickness, are provided with the square recesses above mentioned, and are permanently fixed to the lower three stampings. The upper three stampings are fixed together and can be lifted so as to allow of the insertion of the test piece and then dropped into position, thus closing in the test piece which is then surrounded by the metal to a depth of 1 cm. at each end. Surrounding the ring is a coil of insulated copper wire which is wound on a circular former sufficiently wide near the air-

* "Phil. Trans." R. S., A. Vol. 219 (Appendix), 1919.

gap, to allow of the tilting of the three upper stampings, and also provided with a gap sufficiently wide to allow of the insertion of the specimen. Counting from innermost to outermost the turns of wire, which is wound in three layers, are 146, 140, 126, making a total of 412. The copper wire has a diameter of 0·813 mm., insulated with a double layer of cotton, and the resistance of the winding is 2·25 ohms.

Calculations have been made to find the relation between the current in amperes (C) in the winding and the magnetising force H, produced in the gap. The total line integral of the magnetising force due to the current C is connected with the magnetic induction when no specimen is in the gap by the following equation :—

$$\frac{4\pi nC}{10}=\frac{l_1 I}{A}+l_2 f\left(\frac{\nu I}{A_2}\right),*$$

where n is the number of turns on the ring=412,
l_1 is the length of the air-space=2 cm.,
A is the area of the air-space,
l_2 is the mean length of the lines of magnetic force in the ring=33·9 cm.
A_2 is the area of cross-section of the ring.
ν is the ratio of the induction in the ring to the induction in the air-space.
f is the function defining the magnetising force in terms of the induction in the case of stalloy.

Assuming that the effective area of the air-space is equal to the area of cross-section of the ring, and that the leakage coefficient $\nu=1$, indicating that there is no leakage of the lines of magnetic force, the above equation becomes

$$\frac{4\pi nC}{10}=l_1 \,.\, B_1+l_2 f(B_2),$$

where B_1 and B_2 are the magnetic inductions in the air-space, and ring respectively. In Table I. the function f which defines the magnetising force H_2 in terms of the magnetic induction B_2 is given in the first two columns.† From the assumptions made it follows that $B_1=B_2=H_1$ the magnetic force in the air-space. From the values of H_2 and the mean length, l_2, the line integral of the magnetising force in the stalloy has been calculated. For the air-space the values of $l_1 H_1=l_1 B_2$ have

* "Phil. Trans." R.S. 1886, p. 331.
† Roy. Soc. "Proc." A. Vol. LXXX., p. 548, 1908.

been found, and by addition the total line integral, which is equal to $\frac{4\pi nC}{10}$ has been obtained. In this manner the relation between the current C and the magnetic force H_1 has been

TABLE I.—*Stalloy Magnet Ring.*

Curve of induction for stalloy.		l_2H_2 $l_2=33\cdot9$ cm. stalloy.	No specimen in air-gap.			Ratio of force in gap to amperes H_1/C	Ratio $l_2H_2/l_1H_1+l_2H_2$ per cent.
H_2	B_2		l_1H_1 $l_1=2$ cm	$l_1H_1+l_2H_2$	Amperes C		
			Air.				
0·000474	0·1267	0·0161	0·253	0·269	0·00052	244	5·99
0·000739	0·1918	0·0251	0·384	0·409	0·00079	243	6·14
0·00267	0·674	0·0905	1·35	1·44	0·00278	235	6·28
0·00357	0·937	0·121	1·87	1·99	0·00385	243	6·08
0·00695	1·870	0·236	3·74	3·98	0·00769	243	5·93
0·01286	3·60	0·436	7·20	7·64	0·0148	243	5·71
0·0251	8·25	0·851	16·5	17·35	0·0335	246	4·91
0·0358	13·02	1·214	26·0	27·21	0·0526	247	4·46
0·080	38·0	2·710	76·0	78·71	0·152	250	3·44
0·157	94·1	5·32	188	193·3	0·374	252	2·76
0·245	171·0	8·31	342	350·3	0·677	253	2·37
0·312	269·0	10·6	538	548·6	1·06	254	1·93
0·420	629·0	14·2	1,260	1,274	2·46	256	1·11
0·506	1,063	17·2	2,130	2,147	4·15	256	0·80
0·677	2,245	23·0	4,490	4,513	8·72	257	0·51
1·354	6,050	45·9	12,100	12,146	23·5	257	0·38
2·130	8,200	72·2	16,400	16,472	31·8	258	0·44
3·26	9,810	110·5	19,600	19,710	38·1	258	0·56
5·71	11,500	194	23,000	23,190	44·8	257	0·836
16·20	13,480	549	27,000	27,550	53·2	253	1·99

$4\pi nC/10=l_1B_1+l_2f(B_2)$. Assumes area of air-space and stalloy are equal and that there is no leakage.

$l_1=2$ cm. $l_2=11\cdot43\pi-2=35\cdot9-2=33\cdot9$ cm. $n=412$ No. 21 S.W.G. copper wire

calculated, and their ratio should be a constant if the line integral in the stalloy were vanishingly small. The figures show that the ratio is a maximum when the magnetising force is such that the permeability in the stalloy is a maximum, and that it does not seriously deviate from a constant value.

4. *Experimental Details.*

In the earlier stages of the investigation a galvanometer of the needle type was employed, but owing to the disturbances set up by the ring magnet (even when remote from the galvanometer) and to other local causes, an instrument of the moving-coil type was substituted. This galvanometer had a resistance of 25.7 ohms and a periodic time of 5·95 seconds,

and its sensibility was such that a steady deflection of one scale division at a distance of one metre was produced by a current of $3{\cdot}77 \times 10^{-8}$ ampere.

An exploring coil consisting of 120 turns of fine silk-covered copper wire was wound on a square former of thin card, which allowed of the specimen being inserted with considerable closeness of fit. The coil was connected to the galvanometer, and an adjustable resistance was included in the circuit to limit the deflections. Deflections were obtained on reversal of the magnetising current C, firstly with the exploring coil supported on a wooden core having the same dimensions as the actual specimens, and secondly when the wooden core was replaced by the test piece itself. The ratio of the deflections, allowing for the alteration in the resistance when necessary, and correcting for the air space occupied by the card former, has been taken to be the permeability; and hence the susceptibility has been obtained.

The assumption above made as to the ratio of the deflections is justified on the following considerations. Firstly, an exploration of the magnetic field in the gap showed that it was substantially constant over the whole area. It was in fact about 5 per cent. weaker in the region near the edge of the air space than at the centre. On reversing a current of 1·02 ampere in the magnetising coil the value of H, the magnetic force in the air space was found to be 248 C.G.S. units. Thus the ratio of this force to the current is 244, and Table I. indicates a ratio of about 254. It does not appear necessary to give other illustrative cases, and it is sufficient to notice that a fair agreement can be obtained. As a further test the susceptibilities of certain specimens were obtained by the magnetometer, and when placed in the ring magnet substantial agreement was found. For example, a piece of Manchurian magnetite, obtained by permission from specimen 9,500 in the collection of the Geological Survey and Museum, when tested in the magnetometer with a force of 53 C.G.S. units, had a susceptibility of 0·118. When tested in the ring magnet, the susceptibility was found to vary from 0·11 to 0·12 when the magnetic force had about the same value.

This method of inserting the test piece in a magnetic circuit as above described and testing it for susceptibility, obviously depends to some extent for its accuracy upon the closeness of fit with the pole-pieces, and the authors do not wish to claim for the method a greater accuracy in the result than

TABLE II.

$H_{max.}$	Traversella Piedmont, No. 712.	New York.	Hey Tor, Devon.	Penryn, Cornwall, 242A.	Penryn, Cornwall, 242C.	Arkansas, U.S.A., No. 1.	Arkansas, U.S.A., No. 2.	Altenfjord, Norway.	Lake Champlain New York.	South Manchuria, No. 9,500.	Magnetite-calcite, Aran.	Manganese Steel.
1·25	1·68	...	...	...	...	...	...	...	...	...	...	...
3·75	2·06	0·566	...	...	...	...	...	...	...	...	...	...
10·5	2·47	0·892	0·52	0·235	0·1515	0·139	0·138	0·133	0·119	0·085	0·104	0·226
17·0	3·07	1·251	0·668	0·246	0·155	0·179	0·163	0·147	0·147	0·105	0·108	0·272
27·2	3·09	1·44	0·741	0·265	0·162	0·186	0·169	0·171	0·151	0·105	0·117	0·272
56·0	2·74	1·37	0·892	0·279	0·183	0·207	0·179	0·169	0·147	0·107	0·118	0·311
84·0	2·44	1·25	0·805	0·341	0·204	...	0·195	0·217	0·151	0·122	0·1275	0·387
112·0	2·09	1·108	0·738	0·382	0·222	0·236	0·205	0·248	0·148	0·117	0·119	0·428
175·0	1·51	0·89	...	...	...	...	...	...	...	...	...	...
203·0	1·37	0·816	0·55	0·487	0·27	0·313	0·277	0·27	0·131	0·124	0·118	0·527
257·0	1·14	0·702	0·469	0·492	0·293	0·352	0·33	0·259	0·127	0·116	0·109	0·535
388·0	0·89	...	0·354	0·437	0·30	...	0·327	0·218	0·113	0·106	0·099	0·433
525·0	0·60	0·43	0·287	0·371	0·273	0·305	0·292	0·188	0·093	0·097	0·085	0·376
756·0	0·45	...	...	0·298	...	0·268	...	...	...	0·09	...	0·310

TABLE III.

No.	Description.	Sp. gr. of specimen.	$H_{max.}$	Deflection on reversal θ.	Deflection on break θ_1.	$\theta/2-\theta_1$ / $\theta/2$ per cent.	Magnetising force at max. suceptibility.	Max. susceptibility.	Coercive force for $H_{max.}$ $=525$.	$B_{max.}$ for H $=525$.	Ergs per cycle per cb. cm. for $H_{max.}$ $=525$.	Intensity of magnetisation retained after being in a field of 18,000 C.G.S. units. After 3 hours.	After 6 days.
712	Traversella (crystal) ...	5·06	567	260	114	12·3	22·5	3·12	12·2	4,495	23,200	2·81	1·84
...	New York... ...	4·86	569	325	135	16·7	31·5	1·46	16·8	3,270	14,000	7·45	6·75
...	Hey Tor, Devon ...	4·30	525	235	99	15·4	49·0	0·90	23·8	2,528	15,300	7·6	2·3
242A	Penryn, Cornwall...	4·56	525	260	80	38·5	236·0	0·49	110·0	2,680	81,600	60·6	57·8
242C	Penryn, Cornwall...	4·59	525	227	76	32·8	368·0	0·31	95·0	2,300	58,000	48·2	48·0
1	Arkansas	4·68	525	247	74	39·8	298·0	0·363	150·0	2,580	89,600	69·7	66·3
2	Arkansas	4·74	525	238	72	39·5	315·0	0·348	150·0	2,450	81,800	68·7	65·2
...	Altenfjord, Norway	4·02	525	239	94	21·0	175·0	0·272	65·0	1,760	13,900	27·8	24·7
...	Lake Champlain, New York ...	4·14	525	221	102	7·3	43·8	0·172	15·2	1,112	720	3·08	2·34
9,500	S. Manchuria ...	3.40	525	349	160	8·2	140·0	0·127	31·0	1,132	1,660	6·12	5·35
W30	Magnetite-calcite, Aran	3·40	525	288	141	2·1	85·0	0·129	12·1	1,080	151	1·89	1·14
...	Manganese steel ...	7·68	788	221	70	36·4	228·0	0·54	108·0	3,012	68,800	62·0	49·7

that to which it is entitled. It is difficult with rock specimens to obtain intimate contact at all points between them and the metal, as they are easily broken or chipped. The procedure was first to cut the test piece from the hand specimen—and in this connection we wish to express our thanks to Dr. W. G. Gordon, Head of the Geological Department of King's College, London, for allowing us to make use of his rock-cutting apparatus—and then to grind down the specimen with carborundum powder in water until it just entered the gaps in the pole-pieces. In this manner very fair contact was made and the

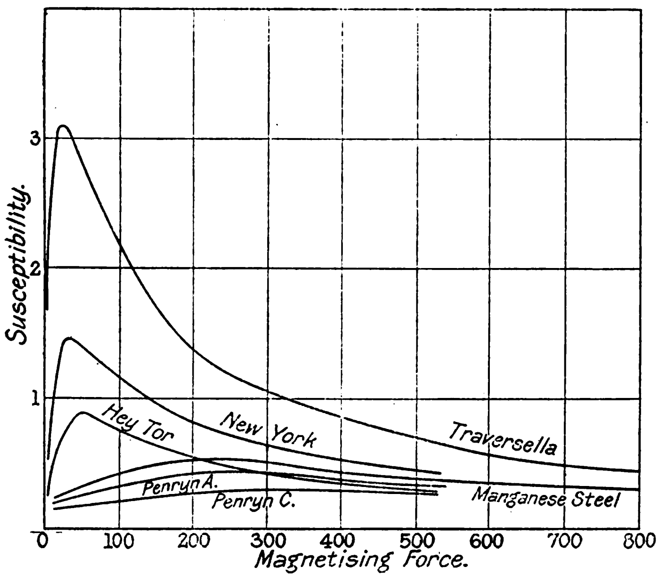


FIG. 1.—K–H CURVES FOR MAGNETITE.

tests show that the results are reliable. Specimens, on being taken out and replaced, when either turned over or turned end to end, gave very closely the same result.

The results obtained are tabulated in Tables II. and III., and illustrated by curves in Figs. 1 and 2. Table II. gives the susceptibility (κ) for all the specimens examined, together with the values of the magnetising force H in C.G.S. units. Table III. gives the specific gravity of the bars actually used in the ring magnet. It also gives information as to the retentivity, maximum susceptibility and the force at which it

occurs, coercive force, dissipation of energy due to the reversal of the magnetising force, and finally the variation of the intensity of magnetisation retained after an application of a force of about 18,000 C.G.S. units obtained by placing the specimen between the coned pole pieces of a powerful electro-magnet. The specimens will now be considered in detail, roughly in the order of their maximum susceptibility.

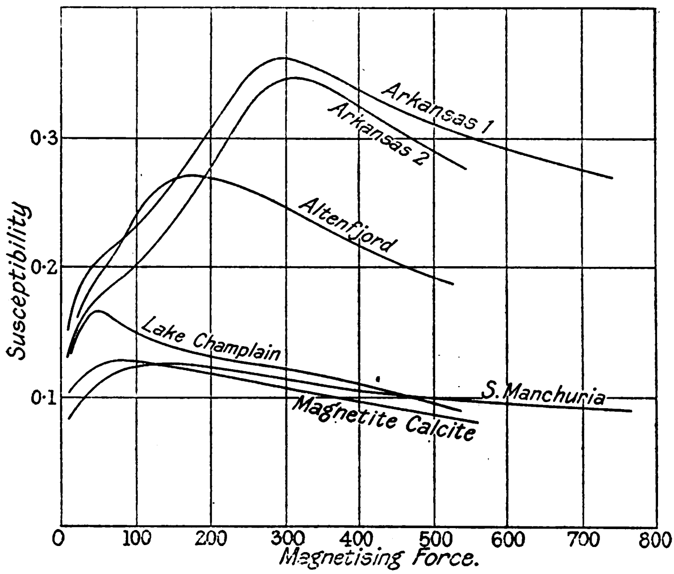


Fig. 2.—K–H Curves for Magnetite.

Traversella, Piedmont. *No.* 712, *G. S. & M.*

The specimen bar was cut (by permission of the Director of the Geological Survey and Museum) at right angles to the principal axis of a large dodecahedral crystal. This material was extremely brittle with almost vitreous fracture exhibiting a bright semi-metallic lustre. Its specific gravity, 5·06 is the highest of any of the various magnetites examined. It also furnishes the highest figure for susceptibility giving a maximum of 3·12 for a force of H=22·5. Its coercive force 12·2 is the smallest of any of the specimens of true magnetite and in consequence its retained magnetism is the lowest.

The above value for the susceptibility is considerably below the results obtained by P. Weiss, as exhibited in his curves

of magnetisation. This may possibly be attributed to a difference in the nature of the specimen.

Small fragments after heating in air to about 1,000°C. appeared to undergo no material alteration in magnetic properties, but the authors have not been able to carry out experiments on a sufficiently large test-bar to decide this point fully.

The values obtained using magnetising forces ranging from 1·25 to 756 C.G.S. units will be found in column 2 of Table II., and the curve showing the variation of K with H in Fig. 1.

New York State. (Exact locality not known.).

This is a very uniform material of specific gravity 4·86, very hard and compact with no indication of any cleavage planes. Its fracture is irregular, bright with numerous glistening points and semi-metallic lustre. It is characterised by a high maximum susceptibility 1·46 and small coercive force, coming in these respects second to the Traversella crystal. As will be seen from the table its permanent magnetisation only amounted to $I=7{\cdot}45$, notwithstanding that while in the field of the electromagnet, the value of B in this material is very high.

After having been raised to a temperature of about 1,000°C. and allowed to regain atmospheric temperature, the magnetic susceptibility for a given force ($H=53$) was found to be unchanged; and there was no measurable alteration in its specific gravity.

Hey Tor, Bovey Tracey, Devon.

A specimen belonging to Prof. H. Louis, of Newcastle-on-Tyne, exhibited great irregularity in the distribution of magnetite. The latter consisted of minute crystals mixed with earthy material. Its fracture was dull and earthy with some sparkling points due to the small crystals. The density varied considerably in different parts from 3·89 to 4·30.

Its susceptibility was as high as 0·9 for forces of about 50 units, and its coercive force relatively small (23·8). The magnetite which it contains belongs therefore to the class having high susceptibility and but little retentivity, the maximum value of I being only 7·6 three hours after having been placed in a field of 18,000 units; this fell in four days to only 2·3. Heated to about 1,000°C. in air and allowed to cool, it showed no tendency to disintegrate and only lost 0·38 per cent.

of its weight, which loss may have been water in its pores, and it showed no change in specific gravity. But although there was very little alteration in appearance or weight its susceptibility for a given force was found to have decreased 11·4 per cent., and its residual magnetism to a somewhat greater extent. This furnishes an example of a magnetite that distinctly diminishes in susceptibility and coercive force after being strongly heated and is an interesting contrast to the next variety.

Penryn, Cornwall. No. 242 *G. S. & M.*

The specific gravity of different pieces from the same specimen varied from 4·50 to 4·65. It is a hard, strong material with a dull fracture and contains small inclusions of a red-brown softer substance, but otherwise compact. This softer substance was found to consist mainly of ferric oxide and ferrous carbonate. When tested in its natural state, its maximum susceptibility had the value of 0·31 for a force of 368 C.G.S. units. After heating to about 1,000°C. the susceptibility rose to 0·492 for $H=235$. In order to exclude any possibility of reduction by furnace gases specimens were heated in an electric furnace in air for 10 minutes to a temperature of 1,000° C. and showed the same marked increase in susceptibility, the maximum of which occurred at a smaller force than for the unheated specimens.

As this kind of magnetite was the only one amongst the many varieties examined by the authors which showed a marked increase in susceptibility, it was thought probable that this might be due to the conversion of some of the ferrous carbonate and ferric oxide existing, as above mentioned, as small inclusions throughout its mass, into magnetic oxide on heating to 1,000°C.

This view was rendered the more probable from consideration of the result of an experiment in which a specimen of this magnetite was heated to about 520°C. for 100 hours to ascertain if prolonged heating to just below the temperature at which magnetite loses its magnetic properties would produce any change. It was found that not only did it fail to show any appreciable increase, but when afterwards it was heated for one hour to 650°C., it showed no further change. Finally, heating it to 1,000°C. in the electric furnace produced no increase in its susceptibility, which after all these various

heatings showed no increase, but had actually diminished about 5 per cent.

As a control two small pieces from the same portion of rock were heated rapidly to 1,000°C. and kept at that temperature for five minutes and allowed to cool in air; they both showed a marked increase in susceptibility of over 60 per cent. The density was slightly raised by this heating process from 4·57 to 4·60, and in a previous case from 4·55 before heating to 4·63 after heating. In these two cases the percentage loss in weight was 1·14 per cent. and 1·07 per cent. respectively. The fact that prolonged heating in air to a temperature of 520°C. prevented the increase in susceptibility may probably be due to the oxidation and decomposition of the ferrous carbonate yielding ferric oxide, while when rapidly raised to 1,000°C. it was transformed (in conjunction with some of the ferric oxide mixed with it) into magnetic oxide (Fe_3O_4). Both the loss of weight and increase of density as well as the great increase in susceptibility receive explanation by this theory, with which the changes of colour and appearance also agree.

G. Folgheraiter* examined the effect of heating on various volcanic rocks and found in general an increase in magnetic properties especially in the amount of retained magnetism, and considered this to be due partly to the conversion of non-magnetic into magnetic substances, and partly to the orientation of magnetite crystals. Brunhes and David† have noticed that the flow of hot lava over clay transformed it into a brick-like material that is magnetic.

With a view to testing the effect of heat upon known material a mixture of kaolin with an equal weight of finely powdered native ferrous carbonate (Chalybite) was made into a stiff clay with water, moulded into bars, dried and finally heated gradually to a temperature of about 900°C.

A rectangular bar 4 cm. × 1 cm. × 1 cm. after being baked was used in the ring magnet and gave a value for K of 0·0224 in a field of 284 units, and 0·021 for $H=178$. Before heating the value of K was too small to measure by this method, but by the use of the magnetic balance it was found to have a susceptibility of 0·000218. Hence its susceptibility was increased about 100-fold by heating to a bright red heat.

In one experiment with a cut bar of this Penryn magnetite the specimen was heated to 1,000°C. and allowed to cool in a

* "Rend. Acc. dei Lincei," Jan. 4 and Feb. 4, 1895.
† "Comptes Rend." 137, p. 975, 1903.

vertical position standing on a piece of asbestos cardboard. When cold its lower end was found to be, as was to be expected, a N-seeking pole, and the intensity of magnetisation it had acquired under the influence of the vertical component of the earth's field was found to be 1·48 C.G.S. units. Experiments conducted on the same bar before heating showed that it required a force of 95 units to impart nearly the same (1·40) intensity of permanent magnetism. Evidently during the cooling process the susceptibility reaches a high value and the magnetism then induced is retained permanently after cooling.

This fact may have an important bearing on the question of the intensity of magnetisation of beds of magnetite in a natural state.

The values for the susceptibility with different magnetising forces for this kind of magnetite are given in columns 5 and 6 of Table II., A being the substance after having been heated to 1,000°C. and C being obtained from a bar in its natural state, unheated.

Arkansas (*I. & II.*).

Two specimens obtained from independent sources (one of the specimens was kindly given to the authors by Dr. Gordon) showed a marked similarity in properties. It consists of a hard compact material with irregular cleavage planes and almost vitreous fracture in some directions while exhibiting a silky lustre in others.

The specimen bar called "Arkansas I." had a specific gravity of 4·68 and "Arkansas II." varied from 4·70 to 4·82, the actual bar experimented with having a specific gravity of 4·74. Both samples showed distinct permanent magnetisation when received and portions of a specimen exhibited a high intensity of magnetisation (I.) of the order of 30 C.G.S. units. As will be seen in Table III. the highest sub-permanent magnetisation exhibited by one of the 4-cm. bars amounted to 69·7, but this was after exposure to a field of about 18,000 units. After being magnetised in a field of 806 the intensity of retained magnetism was 44·5 immediately after removal, and fell to 43·1 in 12 hours. It is evident that a magnetisation of even 30 C.G.S. units for I can only be caused either by a field strength enormously exceeding the earth's force, or by intense local forces such as lightning flashes, as suggested by Pockels* in connection with the magnetisation exhibited by

* "Annal. Phys. Chem.," 63, pp. 195-201, 1897.

specimens of basalt, or by molecular changes accompanying heating and cooling under pressures with which we are at present unacquainted.

The coercive force exhibited by both specimens is the same, viz., 150 units for H max.$=525$, and is higher than for any other magnetite the authors have examined.

A specimen heated to 1,000°C. showed a minute increase in weight after regaining room temperature, but only to the extent of 0·08 per cent.; no change in the density was detected. The susceptibility, which for the specimen was 0·202 for a field of 52 C.G.S. units before heating, was found after heating to have increased for the same force to 0·286, being an increase of 41·6 per cent. Its residual magnetism was also greater, but its coercive force was very materially reduced, so that the retained magnetism was less than half that exhibited by other portions of the same unheated specimen after being in an intense field. While the after effects of heating in this case are to increase K and diminish coercive force (as with hard steel), in the case of Penryn magnetite "242" both K and coercive force were permanently increased.

The dissipation of energy in ergs per cycle per cubic centimetre reaches the high figure of 89,600 for this material for $H_{max}=525$ units, and is the highest recorded in Table III. column 12.

Altenfjord, Norway.

A specimen of magnetite from the above locality was kindly placed at the disposal of the authors by Prof. H. Louis. It was found to be essentially an aggregate of magnetite and felspar having an irregular but crystalline fracture. Its density varied from 4·0 to 4·2 following the variation in proportion of magnetite to felspar.

Its susceptibility for different magnetising forces will be found in column 9 of Table II. Its maximum susceptibility (Table III.) reached the comparatively low value of 0·272 for a force of 175 C.G.S. units.

Heating for 10 minutes to a temperature of about 1,000°C. had no subsequent effect on its weight or specific gravity; the susceptibility of a specimen after this treatment was found to be slightly increased, its value for a given force rising from 0·227 to 0·239, or about 5·3 per cent. increase. The retained magnetisation, however, was found to be substantially the same before and after heating, so that there was no appreciable change in its retentivity.

Lake Champlain, New York State.

One of the varieties of magnetite from this locality was supplied to the authors by Prof. H. Louis and was found to be an aggregate of magnetite and iron pyrites; it had an irregular glistening fracture in which the pyrites could be easily detected by its lighter colour. Its density was found to vary very considerably, 4·10 to 4·62 being the limits for different parts of the same hand specimen, the denser parts being visibly richer in magnetite.

Its magnetic values are recorded in Tables II. and III., and its curve $(K-H)$ in Fig. 2. Its low maximum in susceptibility 0·172 is no doubt due to the variable admixture of pyrites.

The effect of heating this substance to 1,000°C. is to disintegrate it to a greater or less extent, owing to the decomposition of the pyrites. There is a slight loss in weight and decrease in density, the actual figures for the latter in one case being 4·62 before heating and 4·60 after. The susceptibility for the same force was found in a specimen which did not disintegrate to any extent to be diminished about 7·5 per cent., the retained magnetisation being similarly reduced.

South Manchuria. No. 9500 *G. S. & M.*

A specimen of magnetite from the above locality was obtained by permission of the Geological Survey and Museum from their collection. It is essentially a schist having a slaty cleavage in which the laminated structure is easily detected. Owing to its comparative poorness in magnetite its specific gravity is the lowest yet dealt with, being only 3·40, and remarkably constant in different parts of the same specimen. For the same reason its susceptibility is the lowest the authors have found for any rock which can be described as "Magnetite." Its susceptibility, which is 0·085 for $H=10{\cdot}5$, passes through a maximum of 0·127 for $H=140$, falling again to 0·09 for $H=756$. As the $K-H$ curve in Fig. 2 shows, its susceptibility is more nearly a constant than any other variety examined. In this connection the Authors have not yet found in any specimen of magnetite so low a value of the susceptibility (0·016) as that given by Allan.* This, however, may be due to the low value of the magnetising force which he employed.

Heating to 1,000°C. in an electric furnace caused a small loss of weight amounting to 0·07 per cent., probably due to

* Loc. cit.

loss of water; the specific gravity was subsequently found to be unchanged and its magnetic properties were not materially affected. There was a very slight increase in susceptibility, but so small that it falls almost within the limits of experimental error.

Its magnetic values are recorded in column 11 of Table II. and also in Table III.

Magnetite Calcite. (*W.* 30) *Aran, Wales.*

A small specimen of the above rock having been supplied to the authors by the Geological Survey and Museum, it has been included in this series, although strictly speaking it can hardly be designated as a Magnetite. It consists of a matrix of calcite in which are disseminated irregularly numerous very small crystals or crystalline grains of magnetite. In spite of the small proportion of magnetite which it contains (a proportion so variable in different parts that an analysis would give only a vague indication) its susceptibility reaches a slightly higher figure than the Manchurian magnetite last mentioned. The small residual and retained magnetisations, the small coercive force—substantially the same as that of the Traversella crystal—all point to the conclusion that what magnetite is present is in the form of the pure crystal.

From the nature of the matrix—calcium carbonate—it is impossible to investigate the effect of very high temperatures on this material.

A 13 *per cent. Alloy of Manganese and Iron.*

A rectangular specimen of this alloy was cut from a ring which was given to one of the authors some years ago by Sir R. A. Hadfield, F.R.S. It was maintained at a temperature of about 530°C. for 100 hours, with a consequent increase in the susceptibility on return to atmospheric temperature. The results of the experiment with this specimen are interesting as they exhibit a similarity to those of some of the specimens of magnetite. In particular, it is interesting to note the relatively high value of the coercive force and considerable retained magnetisation in this annealed manganese steel, as it is commonly asserted without reference to its condition that manganese steel is almost devoid of these properties.

SUMMARY.

The magnetic properties of certain varieties of magnetite as exhibited by crystallised, compact or massive specimens and detached particles have been examined. In each case the susceptibility has been found to vary with the magnitude of the magnetising force after the manner of iron, the relative variation being much more pronounced in the case of those specimens having the higher susceptibility. The maximum susceptibility in the specimens examined occurs at a force ranging from 13 C.G.S. units in the crystal to 368, its magnitude varying from 3·12 to 0·127 C.G.S. units.

The effect of heating has been greatly to increase susceptibility in some cases and in others a negative effect has been produced. In the case of a specimen of Penryn magnetite, the large increase in the susceptibility was traced to the conversion of ferrous-carbonate and ferric-oxide into magnetite.

As bearing upon the intensity of magnetisation of magnetite in a natural state, it may be mentioned this specimen of Cornish magnetite acquired an intensity of 1.48 C.G.S. units on cooling in the earth's field from 1,000°C., whereas in the cold before heating such an intensity required a field of 95 C.G.S. units to impart the same degree of retained magnetisation.

Very high susceptibility in magnetite is never associated with high coercive force or retained magnetisation, the greatest values for the latter exhibited by specimens having an intermediate value of susceptibility of the order of 0·3 or 0·4. Lower susceptibility may be associated with high coercive force, but naturally the retained magnetisation is not very great, owing to the lower maximum of induced magnetisation.

DISCUSSION.

Prof. LOUIS said that he was not competent to discuss the physical problems involved, but was intensely interested in the practical applications of the work done by the authors of the Paper. He congratulated them on having taken a step towards the solution of the very puzzling questions why magnetite differed so widely from all other minerals in its magnetic properties, and why there were such wide differences in these properties between different specimens of magnetite. The most magnetic magnetite that he had ever met with was in one of the higher peaks of the great mass of magnetite that formed the huge hill of Kirunavaara, where the mineral was so strongly magnetic that on breaking it with a hammer the small chips remained adhering to the face of the mineral by magnetic attraction. The suggestion of the authors that such effects might be due to lightning seemed to fit this case very well. He was not at all surprised at the results obtained from the Penryn mineral; this evidently contained spathic ore, and if this latter were heated even to 500°C. without access of air, the carbonate of iron would be converted into a substance approximating to Fe_3O_4 in composition, and this "artificial magnetite" was strongly magnetic; the presence of Fe_2O_3 was not at all necessary for this action to take place. Carbonate of iron is so easily converted into "artificial magnetite" that this method is quite often used on the large scale for separating spathic ore from such minerals as blende; he (Prof. Louis) had found that in this way it was possible to obtain a certain amount of concentration even in the case of such unpromising material as Cleveland ironstone. He assumed that the Lake Champlain specimen must have been heated with free access of air, so that the pyrites present had been completely oxidised, because pyrites

heated in the absence of an oxidising agent became converted into a sulphide approximating to pyrrhotite in composition, which was fairly magnetic. He hoped that the authors would investigate this particular point and determine under what conditions of heating pyrites gave the most strongly magnetic product. The change began at a comparatively low temperature, and the speaker had met with difficulties in the satisfactory concentration of certain iron ores owing to this effect. The high magnetic susceptibility of the specimens from Traversella was especially interesting, because it was to the ore from this locality—a mixture of magnetite and copper pyrites—that electro-magnetic separation on a commercial scale was first applied. It is quite possible that the high susceptibility was due to these specimens having been cut from an idiomorphic crystal. He sincerely hoped that the authors would continue their extremely interesting researches upon the magnetic properties of minerals.

Prof. A. H. Cox communicated the following: I regret that I am unable to be present at the reading of Prof. Ernest Wilson's Paper, but, although I am not qualified to speak upon the purely physical aspect of Prof. Wilson's researches, I should like to be allowed to congratulate him upon the success of his work upon the measurements of small susceptibilities. The designing of a portable instrument that will measure such small susceptibilities marks an advance which will greatly facilitate work on certain problems, the solution of which appears likely to lead to results of great economic importance. The results so far obtained from the investigation of local magnetic disturbances in certain selected areas, have shed a new light upon the underground structure in one of the most important of our concealed coalfields, and encourage the hope that we have in our hands an additional method of attacking the problem of underground geology. The determinations made by Prof. Wilson gave the necessary clue as to which rocks were responsible for causing the magnetic disturbances in those districts. One of the difficulties encountered during the investigation into the origin of the disturbances was the fact that no susceptibility determinations of such weakly magnetic materials as the rocks involved could be carried out during the course of the outdoor work. Before specimens could be tested magnetically a certain amount of preparation was necessary, and they had all to be sent to Prof. Wilson's laboratory. Now, as the result of his researches, it should be possible to carry out susceptibility determinations at the points where the rocks actually occur, thus effecting a great saving of time, and removing one of the great difficulties in further investigations into the relationship between magnetic disturbance and geological structure. If, as seems probable, such investigations lead to results of national importance, the value of Prof. Wilson's work becomes more than of purely scientific import.

Dr. D. Owen said that the authors' experiments gave interesting information as to the nature of the differences of magnetic quality usually found between various specimens of lodestone. The experimental method, though not quite free from sources of error, appeared to allow of a degree of accuracy satisfactory for the purpose; it would, however, be of interest to have the figure of accuracy stated. In interpreting Figs. 1 and 2 it should be noted that the values of the magnetising force must not be taken too literally. Since many of the specimens were composed of small crystals of magnetite, it is evident that demagnetising forces of uncertain extent must have been present. The fact that the specimen of Traversella showed a much lower value of susceptibility than that obtained by P. Weiss is not surprising, in view of the fact that even good samples of the mineral contain a large amount of impurity, of the order of some 10 per cent. The somewhat low density (5·06) of the specimen supports this view. The explanation proposed by the authors to account for the rise of susceptibility of some specimens after subjection to a rise of temperature appeared reasonable. The ferroso-ferric oxide thereby produced is, like magnetite, distinctly ferro-magnetic.

Comparative figures of susceptibility of the artificial and crystalline varieties would be of interest in this connection. In regard to the problem of the high intensity of magnetisation possessed by some specimens of magnetite, have the authors any evidence as to whether a crystal heated above the critical temperature and allowed to cool in a zero magnetic field would show any sign of magnetisation ?

Mr. C. R. DARLING said that magnetic separators were used for other minerals than magnetite, and it was of interest in connection with the effect of heating obtained by the authors, that in some of these cases heating rapidly to 1,000°C. improved the ease with which separation was accomplished.

Mr. POWER said he had been engaged on the magnetic separation of tin and wolfram ores, and had experienced trouble on account of the magnetic product containing about 33 per cent. of tin. On separating this the iron content was found to be very low. Heating to a high temperature did not alter the magnetic properties. Various pickling processes were tried, but, although a little of the iron was got out, there was still enough left to render the tin magnetic. With regard to the pyrites in the Champlain magnetite, if pyrites is heated slowly it is not nearly so magnetic on cooling as when heated rapidly.

Prof. FORTESCUE asked what steel was used in the experiment shown on the table.

Mr. F. E. SMITH said the authors in their formula took the air-gap as being the space between the jaws of the magnet poles. What effect had the recesses in the pole-pieces on the results ? With regard to the magnetic properties of manganese steel, it was well known that this steel had a permeability of approximately unity in weak fields. Employed on the bridge of a ship, for instance, it would be without effect on the compass. If, however, it was subjected to a strong field it became quite magnetic.

Mr. J. GUILD asked if it would not be possible to make observations of the permeability of the specimens at other than air temperatures. The temperature permeability curves would probably show at a glance the answers to many of the points raised in connection with heat treatment, in addition probably to revealing other points of importance.

Prof. E. F. HERROUN, replying to some of the chemical questions that had been raised in the discussion, said that he agreed with Prof. H. Louis that if the Penryn magnetite, or the mixture of kaolin and ferrous carbonate, had been heated out of contact with air, a still greater increase in susceptibility would have resulted ; but that, as all the other specimens had been heated in air, it was a fair and uniform treatment. He also agreed that by heating the magnetite containing pyrites in air some of the latter would be oxidised instead of merely losing some of its combined sulphur. In reply to another question, he said that ferrous or ferric oxides resulting from the decomposition of iron compounds, although paramagnetic, were extremely feeble in comparison with the ferro-magnetic *magnetite*, Fe_3O_4 : crystals of the latter may be formed artificially by using a fusible matrix. Being asked by the President for his distinction between para and ferro magnetic bodies, he said he regarded a ferro-magnetic substance as one which showed a definite K–H curve of susceptibility, while the latter was practically a straight line with paramagnetic bodies for all fields, and its value very small.

Prof. E. WILSON said that Prof. Louis had mentioned how highly magnetic was the magnetite on some of the summits of the well-known Swedish deposit, which had been estimated to contain about 200 million tons. If this magnetisation was due to lightning flashes, it would be interesting to know if the magnetite below the surface was definitely magnetic. In an experiment with bars of Arkansas magnetite and glass-hard steel of the same size, the authors showed at the meeting that, although the bar of magnetite had evidently the greater moment (by its action on a compass needle) its portative force was smaller. In answer to a question by one of

the speakers, he stated that the steel used was a carbon tool steel. He explained that the portative force depends upon the square of the magnetic induction, and that the lower permeability of the magnetite counteracted the higher moment to the extent of making its portative force smaller. Mr. F. E. Smith had remarked upon the value of the permeability of manganese steel being unity, or very nearly so, in weak fields, and that the effect of the earth's field on this steel would not appreciably affect a compass needle. Prof. Wilson said that the value of the permeability of 13 per cent. manganese steel usually quoted was 1·27, and that this figure was obtained by the late Dr. John Hopkinson in the case of a ring. If the surface of the ring is ground away, he had found the permeability of the interior portion had a value of 1·005 in a field of about 100 C.G.S. units. After prolonged heating to 530°C., its permeability may be raised to 8 or 10, or even higher. In regard to Mr. Smith's remark on the influence of the recess in the pole-faces, it is stated in the Paper that the force varies 5 per cent. between the centre of the gap and the edge of the pole-face. If the instrument had to be used for the measurement of very high permeabilities, the closeness of fit would be serious, as is well known in connection with permeameters; but the permeabilities met with in magnetite are not of such a high order as to render the method of test invalid.

XXIII. *The Current-voltage Characteristics of High-voltage Thermionic Rectifiers.* *By* Prof. C. L. Fortescue.

Received May 30, 1919.

1. *Introductory.*

The following notes partake more of the nature of a set of designer's curves than of a record of new work, the conditions prevailing in thermionic rectifiers having been fully described already by Dushman.*

The combination of a high-voltage thermionic rectifier, an alternating-current transformer and a smoothing condenser seems likely to have a wide application as a source of high-voltage direct current. Such an arrangement is well adpated, for example, for X-ray work, for wireless telegraphy and for any laboratory purpose requiring an approximately steady high-voltage unidirectional supply.

The application of these rectifiers for practical wireless work led the author, in collaboration with Mr. C. M. Sleeman, of

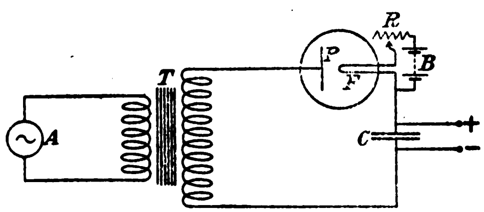


Fig. 1.

Queen's College, Cambridge, to work out the numerical relationships which form the subject matter of this Paper; and, in view of the probability of there being many other applications, it has seemed desirable that the results should be published. These results were required for the development of Wireless Telegraphy in the Naval Service, and this Paper is published by special permission of the Admiralty.

2. *General Arrangement of the Circuits.*

The simplest form of circuit is that shown in Fig. 1. In this figure, A is an alternator and T is the alternating-current transformer necessary to give the high-voltage alternating supply. The thermionic rectifier V consists of the usual

* "General Electric Review," March, 1915.

"plate" or positive electrode, P, and tungsten wire filament, F, for the negative electrode. A high vacuum rectifier is essential for high-voltage working, and the electrodes must be so treated during manufacture that no gas is evolved when the rectifier is in use. The filament is heated by current from a battery, B, which must be highly insulated, or, alternatively, from a separate small alternating-current transformer having a low voltage, but highly-insulated secondary winding. The current in the filament is controlled by the resistance R.

When the filament is at a suitable temperature the electron emission allows of a current flowing during the part of the half cycle when the plate is positive to the filament; but during the rest of the cycle, when the plate is negative to the filament, no current flows so long as the vacuum is sufficiently good. Thus the condenser C becomes charged, and an approximately steady direct current can be taken from the terminals marked + and −. The voltage across the condenser C

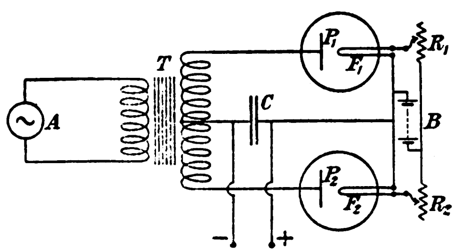


FIG. 2.

will depend upon the secondary voltage of the transformer, and upon the ratio of the direct current supplied to the saturation electron current obtainable from the filament of the rectifier.

An improved arrangement is that shown in Fig. 2, where two rectifiers are used. The operation of this combination is exactly the same as in the previous case, except that the rectifiers come into action alternately, one during one half cycle and one during the other. This arrangement has been described as the "Bi-phase" system of connecting up the rectifiers.

As compared with Fig. 1, the transformer secondary voltage must be twice as great, and the electron emission from each filament one-half, for the same approximately steady voltage and output.

Many other similar circuits can be used. For example, where three-phase power is available, three or six-phase connections, with three or six rectifiers, may be used, with the corresponding reduction of the size of smoothing condenser for a given uniformity of the direct-current supply.

With arrangements of this kind there is no difficulty in obtaining a supply of steady direct current up to values of an ampere, or more, at voltages up to 10,000 volts.

3. *A More Detailed Consideration of the Action of the Circuits.*

Consider the steady conditions in which a steady direct current of I_0 amperes is being supplied at a P.D. of V_0 volts, both current and voltage being approximately uniform.

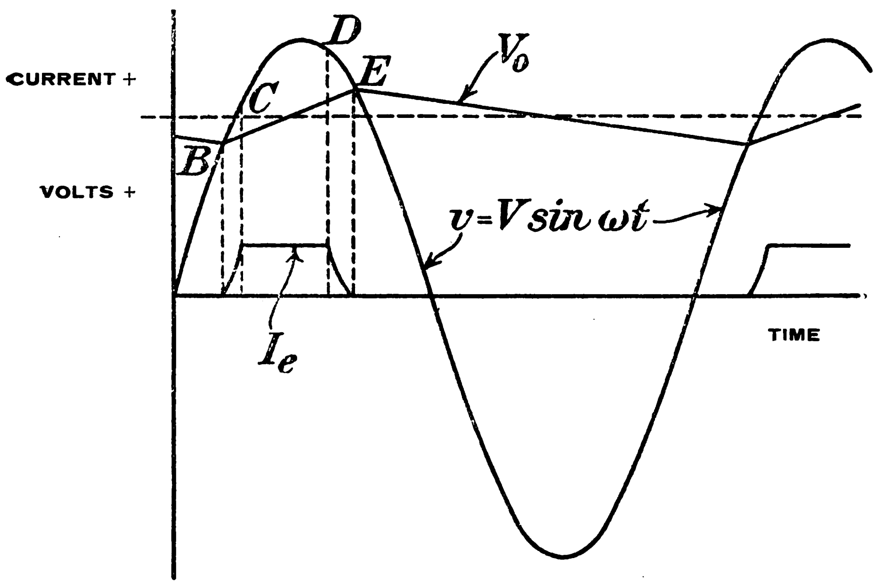


Fig. 3.

Let $v = V \sin \varpi t$ be the transformer secondary voltage, assumed to be approximately sinusoidal, and let I_e be the saturation electron current. The action of the circuit for one cycle is shown diagrammatically in Fig. 3. Until the transformer voltage rises above the condenser voltage V_0, as at B in Fig. 3, no current passes. From B to C the effective voltage across the rectifier is gradually increasing, with a corresponding increase of the current flowing. At every instant the current

varies as the 3/2th power of the excess positive voltage of the plate. At some point, C, however, the saturation value I_e will be reached. From that point the current remains constant until the point D is reached, there being a corresponding small rise of the voltage V_0 during this period. But from the instant D onwards the voltage between plate and filament is falling below that required to produce the saturation current, and consequently from that point onwards the current falls until the instant E is reached, at which there is no voltage between plate and filament, and the current ceases. For the remainder of the cycle there is no conduction through the rectifier, and the voltage at the condenser gradually falls to the value it had at the instant B; after which the process is repeated for the next cycle.

For steady conditions the quantity of electricity passing through the rectifier during the conductive part of the cycle must be equal to the quantity flowing out from the condenser C as a steady current during the whole cycle. Thus, for a given steady supply voltage, V_0, the greater the steady current required the greater must be the saturation current I_e; or, alternatively, for a given value of I_e and of the alternating-current supply, the greater I_0 is, the lower will be the voltage V_0, since the electron current must flow for a longer time to supply the larger quantity per cycle. The maximum value of I_0 is $\frac{1}{2}I_e$, when the voltage V_0 will be zero.

4. *Approximate Calculations.*

The following approximate method has been found to give good results, especially where the values of V and V_0 are both large compared with the voltage V', required to drag away the saturation electron current. In a later paragraph corrections are given which can be applied to the formulæ when a higher degree of accuracy is required.

It is assumed that the periods BC and DE of Fig. 3 are negligibly small, so that the electron current can be supposed to rise to its saturation value at the instant at which the transformer voltage reaches the average steady voltage V_0; and to fall to zero again at the instant when the transformer voltage is again equal to the condenser voltage. Under these assumptions the curve of the current through the rectifier becomes a rectangular one of constant maximum value I_e.

(*a*) *The Capacity Required in the Smoothing Condenser.*—Let the value of the angle ϖt corresponding to the instant at which

$v=V_0$ be θ—*i.e.*, $\theta=\sin^{-1}\frac{V_0}{V}$. Then, if T is the time for one complete cycle of the alternating supply, the full electron current is flowing for a period of $\frac{\pi-2\theta}{2\pi}\,.\,T$ seconds per cycle. The quantity of electricity passing into the condenser during the conductive period is therefore $I_e.\frac{\pi-2\theta}{2\pi}\,T$. The quantity leaving during this period is $I_0\,.\,\frac{\pi-2}{2\pi}\,T$. The quantity to be stored in the condenser for use during the non-conducting period is therefore $(I_e-I_0)\,\frac{\pi-2\theta}{2\pi}\,T$.

But for steady conditions, since the quantity per cycle flowing into and out of the condenser must be the same

$$I_e\,.\,\frac{\pi-2\theta}{2\pi}\,T=I_0T,\text{ or }I_e/I_0=2\pi/(\pi-2\theta).$$

The maximum change of charge in the condenser during the cycle is, therefore, $I_\theta\,.\,\frac{\pi+2\theta}{2\pi}\,T$.

If the permissible variation of the condenser is aV_0, where a is a fraction having values of from about 0·1 downwards, then, if C is the condenser capacity,

$$C=\frac{I_0}{V_0}\,.\,\frac{\pi+2\theta}{2\pi}\,.\,\frac{T}{a}=\frac{I_0}{V_0}\,.\,\frac{1}{af}\,.\,\frac{\pi+2\theta}{2\pi},$$

where f is the frequency of the alternating-current supply.

For example, if $I_0=0{\cdot}05$ amperes, $V_0=10{,}000$, $f=50$, $\theta=1$ radian, and $a=0{\cdot}05$; then $C=1{\cdot}64$ microfarads.

With the bi-phase arrangement, the capacity required is found by a similar argument to be

$$C=\frac{I_0}{V_0}\,.\,\frac{1}{af}\,.\,\frac{\theta}{\pi}.$$

With the numerical values as just previously given, the capacity of the condenser to give the same smoothing effect would be 0·64 microfarads, showing the advantage of the bi-phase system. With the multi-phase arrangements, this advantage is still further accentuated.

The overall dimensions of the condenser are approximately proportional to CV_0^2 which is equal to $\frac{I_0V_0}{af} \cdot \frac{\pi+2\theta}{2\pi}$ or $\frac{I_0V_0}{af} \cdot \frac{\theta}{\pi}$ for the single-phase or bi-phase arrangements respectively. Hence for a given frequency of the alternating-current supply the dimensions of the condenser required for a given smoothing effect are dependent only on the output power. Thus with high power it becomes almost necessary to use either high frequencies, *i.e.*, frequencies above the ordinary commercial frequencies, or to use the multiple-phase systems. As will be pointed out later, there is no loss of regulation or of overall efficiency involved in using the multiple-phase system. It is slightly more complicated in that there are several filaments to be adjusted instead of only one; and the initial expense will be slightly greater, since it costs somewhat more to make two small rectifiers than one large one of the same output. When nearing the limit of the power that can be dealt with in a single rectifier, this latter objection does not hold, however, as the cost of the large rectifiers goes up very rapidly when nearing the limit of size.

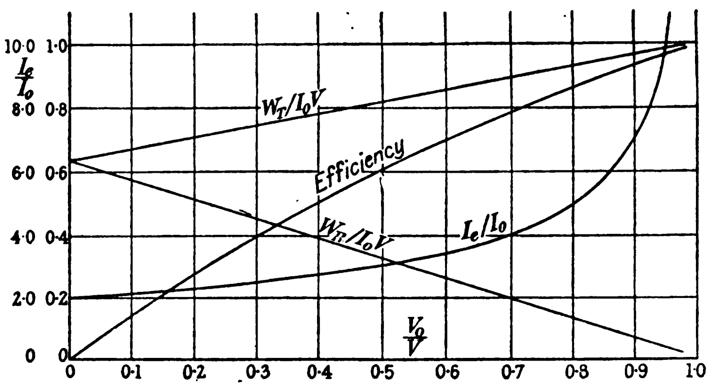


Fig. 4.

(*b*) *The Ratio of the Output and Saturation Currents.*—It has been shown just previously, from consideration of the fact that the quantity of electricity flowing through the rectifier per cycle must be the same as the quantity flowing out of the condenser, that $I_e/I_0 = 2\pi/(\pi-2\theta)$, where $\theta = \sin^{-1} V_0/V$. Hence

for any assigned value of V_0/V and of I_0, the necessary saturation current I_e can be estimated. A curve of the ratio I_e/I_0 in terms of V_0/V is useful, as given in Fig. 4.

(c) *The Power taken from the Transformer.*—On the same assumptions, the power taken from the transformer W_T is

$$W_T=\frac{\varpi}{2\pi}\int_{\varpi t_1=\theta}^{\varpi t_2=\pi-\theta} I_e \,.\, V \sin\varpi t dt=\frac{I_e \,.\, V\cos\theta}{\pi}=I_0V\frac{2\cos\theta}{\pi-2\theta}=kI_0V,$$

where $$k=\frac{2\cos\theta}{\pi-2\theta}.$$

(d) *The Power Expended in the Rectifier.*—The mean power expended in the rectifier is

$$W_R=\frac{\varpi}{2\pi}\int_{\varpi t_1=\theta}^{\varpi t_2=\pi-\theta} I_e(V\sin\theta-V_0)dt=I_0V\left(k-\frac{V_0}{V}\right).$$

(e) *The Rectifying Efficiency.*—Neglecting the power expended in heating the filament, the efficiency of the rectification is $\dfrac{W_T-W_R}{W_T}=\dfrac{1}{k}\cdot\dfrac{V_0}{V}$.

Curves of k, $k-V_0/V$ and V_0/kV can be plotted as shown in Fig. 4. These curves show clearly the compromise that has to be made in practice between the ratio V_0/V and the efficiency on the one hand, and the saturation current on the other.

The curve of $k-V_0/V$ is important as it gives the power that has to be dissipated at the plate of the rectifier for any value of I_0V_0. The power dissipation is one of the fundamentals upon which the design of the rectifier depends.

(f) *The Root-mean-square Current taken from the Transformer.*—The saturation current I_e is flowing in the secondary winding of the transformer for a period $\dfrac{\pi-2\theta}{\varpi}$ seconds per cycle. Then if J is the root-mean-square value of this current

$$J^2\,.\,\frac{2\pi}{\varpi}=I_e^2\,\frac{\pi-2\theta}{\varpi},\ \text{or}\ J=I_e\left(\frac{\pi-2\theta}{2\pi}\right)^{\frac{1}{2}}.$$

It is convenient to plot the ratio J/I_0 for various values of V_0/V as is done in Fig. 5.

5. *The Application of these Results to the Bi-phase and Multi-phase Methods of Connecting up the Rectifiers.*

The action of the bi-phase circuit is shown diagrammatically in Fig. 6. The curves of Fig. 4 can be applied directly to this case if the transformer secondary voltage $v = V \sin \varpi t$ is taken as the transformer voltage between outers and the centre point, instead of the voltage across the whole secondary

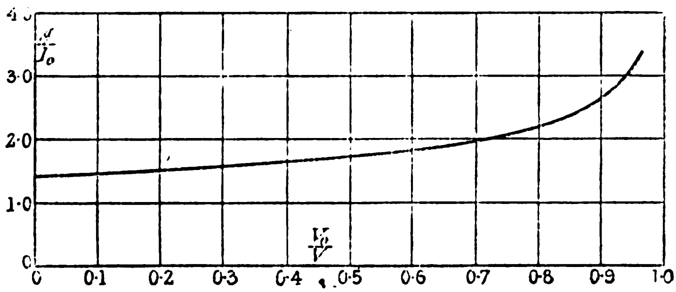


Fig. 5.

winding; and if the ratio I_e/I_0 is halved, I_e being the saturation current per valve, *not* the sum of the saturation currents for the two valves.

For a three-phase circuit the transformer voltage must be taken as between outers and centre point and the ratio I_e/I_0 must be divided by three.

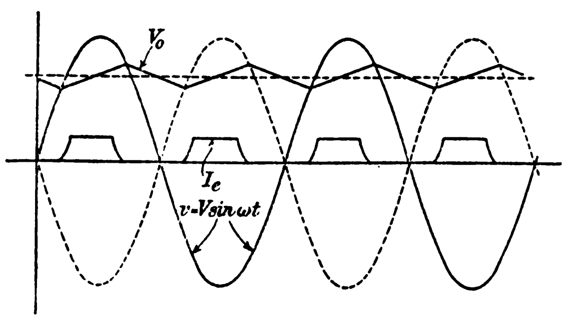


Fig. 6.

In general, for n phases each of voltage $v = V \sin \varpi t$, the curves of Fig. 4 are applicable if the ratio I_e/I_0 is divided by n.

The efficiency will clearly be unaffected by the number of phases because the efficiency curves of Fig. 4 apply to each

individual valve, and therefore to any number of valves, each of which is independent of the rest.

6. *The Over-all Efficiency of the Rectifier.*

From the curves of Fig. 4 it is seen that the rectifier efficiency increases as the ratio I_e/I_0 increases. In practice there is a limit to this ratio arising from the large current required for the heating of the filament and from the large amount of power consumed in the filament. Since the emission per unit area of the filament decreases with decrease of filament temperature, a larger area must necessarily be used with the lower temperature. This means that either the filament must be longer and work at a higher voltage, or it must be of larger diameter and take a larger current.

The characteristics of tungsten filaments have been given by Langmuir.* These characteristics show that the filament watts per ampere of electron emission fall rapidly as the temperature increases. But the life of the filament also falls rapidly as the temperature increases. Consequently, the filament watts per ampere of electron emission is an indication of the life to be expected from the filament of the rectifier.

The overall efficiency of the rectifier, including the filament watts, can be estimated for a series of values of the filament watts per ampere, and overall efficiency curves plotted.

Using the same approximations as before, the output of the transformer is $W_T=kI_0V$. The watts expended in the filament may be expressed as $W_F=wI_e$, where w denotes the watts per ampere of electron emission.

Thus the total input to the rectifier is

$$W=W_T+W_F=kI_0V+wI_e=I_0V\left(k+\frac{w}{V}\;\frac{2\pi}{\pi-2\theta}\right).$$

The output from the rectifier is I_0V_0 and the overall efficiency is, therefore :—

$$\frac{V_0/V}{k+\frac{w}{V}\cdot\frac{2\pi}{\pi-2\theta}}.$$

The curves of Fig. 7 give the overall efficiency in terms of V_0/V for various values of w/V. The falling-off of the maximum efficiency as the ratio w/V increases, is very marked. With a filament temperature corresponding to 100 watts per

* "Physical Review," N.S., Vol. VII., No. 3, March, 1916.

ampere of electron emission, and a transformer peak voltage of 10,000, the maximum efficiency is in the neighbourhood of 90 per cent. Under these conditions an average life of from 1,000 to 2,000 hours would be expected. But if $w=200$ and $V=4{,}000$, the maximum efficiency is only about 65 per cent.

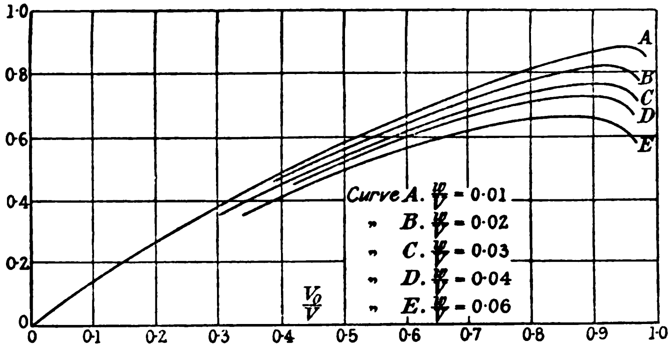


FIG. 7.

A working life of perhaps 3,000 hours would, however, be expected under these conditions.

7. *The Regulation.*

Assuming that for a particular adjustment of the rectifier I_e is constant and that the transformer voltage remains constant, then the " regulation " may be taken to describe the variation of V_0 with I_0.

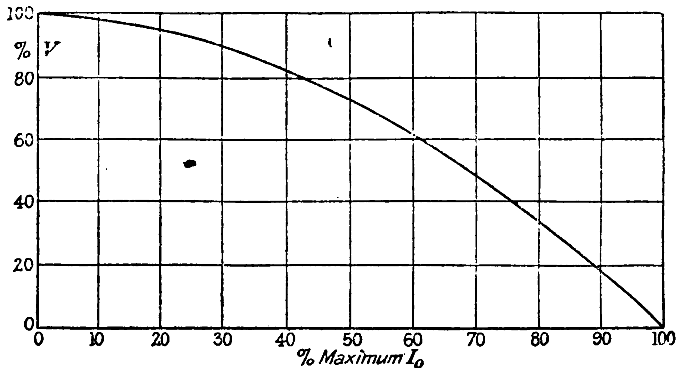


FIG. 8.

This can be determined from the curves of Fig. 4. For every value of the ratio V_0/V the corresponding value of

I_e/I_0 can be found. Thence, giving V and I_e particular values, the corresponding values of V_0 and I_0 can be ascertained.

The maximum value of I_0 is $0{\cdot}5I_e$, and the maximum value of V_0 is V. In Fig. 8 the regulation has been plotted as a curve of the percentage of the maximum value of V_0, viz., V, against the percentage of the maximum value of I_0, viz., $0{\cdot}5I_e$.

8. *The Corrections for the Various Assumptions that have been made.*

(*a*) The assumptions with respect to the periods *BC* and *DE* of Fig. 4.

The assumptions made in neglecting these periods are incorrect in that :—

(i.) The voltage V_0 varies below and above the average value during the conductive period.

(ii.) The electron current rises gradually during the time that the excess plate voltage is less than the voltage required to produce the full saturation electron current.

The effect of the variation of V_0 is that in the early part of the conductive period the electron current starts earlier than is assumed ; and in the latter part of the conductive period the fall of the electron current begins earlier than is assumed. The two effects, therefore, tend to neutralise one another. But since the curvature of the sine wave increases towards the apex of the wave the advance (in time) at which the fall of the current sets in is greater than the advance in time of the point at which the rise of the current begins. The result is therefore that the quantity passing through the rectifier is somewhat over-estimated.

For any given ratio of V_0/V and for any assigned value of a, the angles corresponding to the start of the current and to the fall can be found by reference to a table of sines and circular measure of angles. The error in the estimate of the angle for which the full current is passing can be found and compared with the assumed angle of $\pi-2\theta$. Curves of percentage error for each value of V_0/V can then be plotted. This has been done in Fig. 9. It will be seen that the errors are small except where V_0/V approaches unity, and when $a/2$ is comparable to $1-V_0/V$.

For example, if $V=10{,}000$, $V_0=7{,}000$ and $a=0{\cdot}2$, the error is only just over 1 per cent.

The error arising from the second assumption is very much more serious. The electron current does not rise instantaneously to its full value. For any particular case the result of this supposition can be found by actually plotting the current

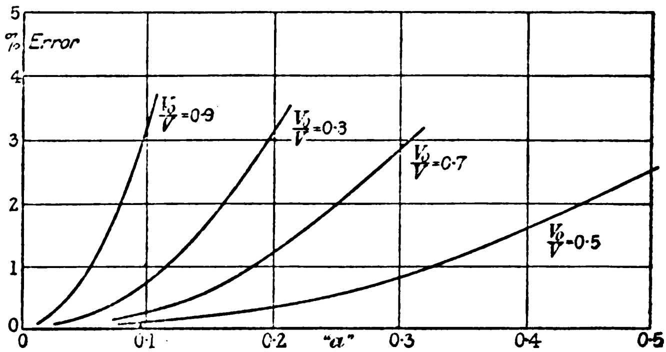


FIG. 9.

curve for the interval of time elapsing before the full current is attained. In Fig. 10 a somewhat extreme case is taken in which $V=5{,}000$, $V_0=4{,}000$, and $V'=500$. The error in the estimation of the quantity is found by taking the area beneath the actual curve and comparing it with the area assumed in

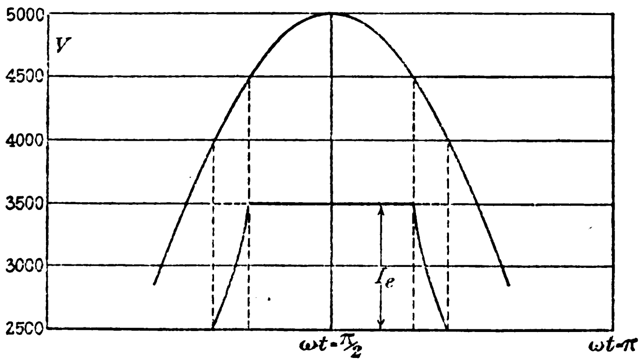


FIG. 10.

obtaining the formulæ. In this case the formulæ give an excess value of about 17 per cent.

Obviously the smaller V' becomes the smaller will be the error. For example, if $V=10{,}000$, $V_0=7{,}000$, and $V'=200$, the error is reduced to something of the order of 2 per cent.

The error introduced in this way can be estimated with a fair degree of accuracy, as follows :—

So long as V' is not very large, the value of $V-V_0\,(=v')$ may be taken as varying directly as the time. and may be written $v'=k't$, where $k'=\left(\frac{dv}{dt}\right)_{\varpi t=\theta}$.

Then at every instant the electron current $(=i')$ is $i'=Av'^{\frac{3}{2}}=A't^{\frac{3}{2}}$; A and A' being constants.

The quantity passing before the saturation value is reached is, therefore,

$$\int_0^{t'} i'dt=\frac{2}{5}A't'^{\frac{5}{2}}=\frac{2}{5}I_et',$$

where

$$t'=\frac{1}{\varpi}\left(\sin^{-1}\frac{V_0+V'}{V}-\sin^{-1}\frac{V_0}{V}\right).$$

When deriving the formulæ it was assumed that the full current I_e was flowing for $(\pi-2\theta)/\varpi$ secs. per cycle.

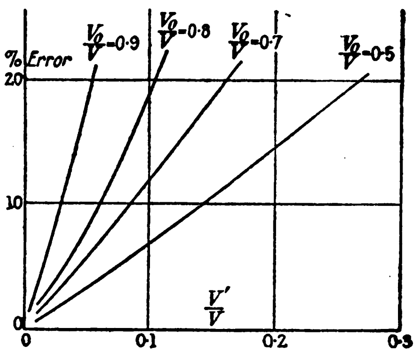


FIG. 11.

But during the initial and final periods the quantity is only $\frac{2}{5}I_et'$, instead of I_et' as assumed. The over-estimate is therefore $\frac{3}{5}I_et'$, which, expressed as a percentage error, is

$$120\frac{\varpi t'}{\pi-2\theta}=\frac{120}{\pi-\sin^{-1}V_0/V}\left(\sin^{-1}\frac{V_0+V'}{V}-\sin^{-1}\frac{V_0}{V}\right).$$

For various values of V_0/V, a series of values of V'/V can be taken, and the curves can be plotted as in Fig. 11.

From these curves it will be observed that the greatest errors correspond to the highest values of V_0/V.

For any assigned values of V and V' both these percentage corrections may be applied to any desired point on the curve of I_e/I_0 of Fig. 4.

The root-mean-square value of the current taken from the transformer is altered by these corrections in very approximately the same proportion as the current I_0.

(*b*) The error arising from neglecting the inductance and resistance of the transformer.

It has been assumed that the voltage applied to the rectifier circuit is a sinusoidal voltage $v = V \sin wt$. In practice the supply may be from alternating current mains or from an alternator. In either case the inductance and resistance of the leads, alternator and transformer can be converted into equivalent inductance and resistance in the high voltage side, in the usual way. The conditions are then represented by a sinusoidal E.M.F. applied to a rectifier circuit similar to that of Fig. 1, but with this equivalent resistance and inductance in series.

Both the inductance and the resistance will have some effect on the voltage-current characteristic of the rectifier.

So long as the inductance is not very large the resulting effect is relatively small. Regarding the current through the rectifier as an impulse, it follows that the initial increase from zero to the saturation value I_e will be delayed. But when the end of the conductive period is reached the falling-off of the current will be similarly delayed. The exact behaviour in any particular requires rather laborious numerical work, but to a fairly near approximation it may be assumed that the two effects neutralise one another, so long as the voltage across the inductance during either the BC or DE periods of Fig. 2 is not much greater than the voltage required to drag away the saturation electron current.

The resistance of the circuit has, however, a cumulative effect, in that it delays the rise of the current and advances the fall. Again, it is difficult to derive a formula to meet all cases, and results are most easily obtained by graphical methods. The rising and falling parts of the curves can be plotted on the assumption of no resistance, as in Fig. 10. For each value of the current in these curves the IR voltage drop can be calculated, and from the voltage wave the instant at which the excess voltage is reached can be found. By this means a corrected curve of the rise of I_e can be plotted, and the error estimated by taking the areas. The curve of

Fig. 12 corresponds to the curve of Fig. 10, but a correction for a resistance of 500 ohms has been taken, the saturation current being 0·5 amperes, and the potential difference required to drag this current away being 500 volts. It will be observed that the ratio of the errors due to the voltage drop in the resistance and to V' are roughly proportional to the values I_eR and V'. Thus the curves of Fig. 11 can be used as a close approximation for the correction for the resistance drop, by simply adding on to V' an amount equal to I_eR.

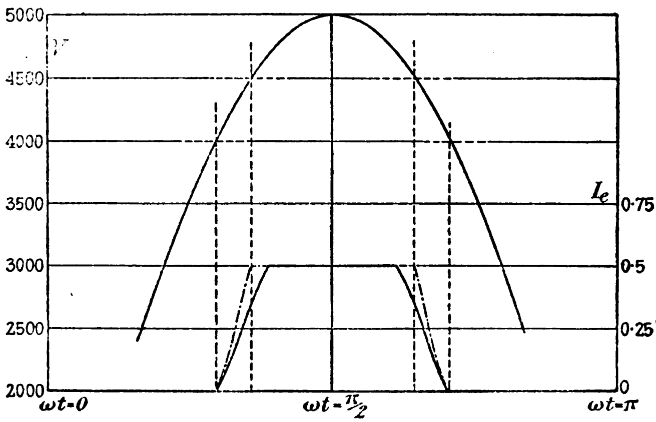


FIG. 12.

9. *Numerical Example.*

A steady current supply of 0·1 ampere at 6,000 volts is required from a 220-volt 50-cycle alternating current supply, a probable working life of the rectifiers of not less than 2,000 hours being provided for and the steady voltage variation being reduced to 1 per cent.

A probable working life of 2,000 hours for the rectifiers means with tubes of present design, that the watts per ampere of electron emission will be at least 100, and probably more. To provide some factor of safety 150 watts per ampere may be taken.

Inspection of the curves of Fig. 7 shows that a ratio of V_0/V for maximum efficiency will be in the neighbourhood of 0·9. Thus, if $V_0=6,000$, V will be approximately 6,700 and $W/V=150/6,700=0·0225$, for which the maximum overall efficiency will occur very near to the point at which $V_0/V=0·9$.

Z 2

If the voltage ripple is not to exceed 1 per cent. with this low-frequency supply, a bi-phase circuit will certainly be used.

The transformer voltage step-up will therefore be from 220 to 9,400 volts root-mean-square. Some allowance must be made for the resistance of the windings, and a step-up of 220/10,000 would probably be specified so as to be a little on the safe side.

Referring to Fig. 4 the ratio I_e/I_0 for $V_0/V=0{\cdot}9$ is about 6·8.

In determining I_0, some allowance must be made for the corrections given in Fig. 11. As a first approximation these may be taken as 20 per cent. Thus, a total I_0 of 0·12 ampere should be calculated for.

Using the bi-phase arrangement an electron emission of $0{\cdot}12\times 6{\cdot}8/2=0{\cdot}41$ amperes would have to be provided for.

The energy to be dissipated in each rectifier is found from Fig. 4 by scaling off the value of W_R/I_0V corresponding to $V_0/V=0{\cdot}9$.

With the bi-phase arrangement the maximum voltage per tube is 7,070, and the effective value of I_0 is 0·12/2. Hence, $W_R=0{\cdot}06\times 7{,}070\times 0{\cdot}16=68$ watts.

The root-mean-square value of the current in the transformer windings is found from Fig. 5. The ratio of J/I_0, corresponding to $V_0/V=0{\cdot}9$ is 2·7. This is for a single-phase rectifier.

With the bi-phase arrangement the value of I_0 to be taken for one tube is one-half of the total.

Hence, $=2{\cdot}7\times 0{\cdot}12/2=0{\cdot}162$ ampere.

The corrections must now be considered. The voltage required for the saturation current of a tube of this output will be given by the manufacturers, and will probably be about 150 to 200 volts. Assuming $V'=200$, $V'/V=200/7{,}070=0{\cdot}3$ about.

Hence, the value of I_0 will be over estimated by about 12 per cent. The other corrections are relatively small, and need not be considered unless a very high degree of accuracy is required.

The proposed design, therefore, appears to be a little on the liberal side all through.

Whether a recalculation to meet the conditions more exactly should be carried out depends upon the conditions. If they are exactly those required this design would stand. If they in themselves involve a factor of safety, then a recalculation would be made.

It now remains to determine the capacity of the smoothing condenser.

It is specified that the ripple shall not exceed 1 per cent.—*i.e.*, $a=0{\cdot}01$.

The capacity required is given by

$$C=\frac{I_0}{V_0}\cdot\frac{1}{af}\cdot\frac{\theta}{\pi},$$

where $$\theta=\sin^{-1}\frac{V_0}{V}=\sin^{-1}0{\cdot}9=1{\cdot}12.$$

$$\therefore\qquad C=\frac{0{\cdot}1}{6{,}000}\cdot\frac{1}{0{\cdot}01\times 50}\cdot\frac{1{\cdot}12}{\pi}=12\times 10^{-6},$$

i.e., 12 mfd.

The outline specification of the apparatus required is thus :—

Rectifiers.—Two in number required, designed to give an electron emission of 0·41 ampere with an effective life of 2,000 hours when operating on a circuit of 5,000 volts root-mean-square. The arrangement of the leads through the glass and supports for the electrodes must be such that a voltage of 13,000 volts, with the filament positive, will be safely withstood. The plate to safely withstand an energy dissipation of 68 watts continuously without appreciable evolution of occluded gas.

Transformer.—Wound for a step-up of from 220 to 10,000 volts, the windings being designed for a root-mean-square secondary current of 0·162 ampere. Three secondary terminals are required, one at each end of the winding and one at the mid-point.

Condenser.—To be of capacity 12 mfd., and to be insulated for 6,000 volts.

Summary of Contents.

The Paper refers to the use of high-voltage thermionic tubes for the production of high-voltage steady current from an alternating-current supply. The subject is approached from the designer's standpoint. Curves and approximate methods of calculating them, are given from which the best combination of electron current and alternating supply voltage can be determined for any prescribed conditions.

Further, the curves allow of the essentials of the specification of the rectifiers and transformer being ascertained, and a formula is given for the capacity of the smoothing condenser

required to maintain the steady supply voltage within any assigned limits. Finally, correction curves are added by means of which more exact results can be obtained from the approximate curves given in the first instance.

DISCUSSION.

General G. O. SQUIER thought the Paper was of great interest and provided an efficient means of obtaining high-voltage direct current. He was glad to see the large number of Papers to the Physical Society dealing with the thermionic tube. The development of this subject during the war had been very rapid—at least equal to 10 years' ordinary progress. In the United States over a million per annum of these valves are now being turned out. This large output had forced the standardisation of sockets and other parts, which in itself was a very useful result. The increasing use of the valve was evident every day, and it was difficult to foresee how far it would ultimately displace existing apparatus. It was so simple and adaptable in use that it would play a leading part in the future development of physics. He knew of no other case in which there was a smaller time-lag between laboratory investigations and everyday use than in the case of the thermionic tube.

Prof. BRAGG said the Paper dealt with a problem which was of great interest to physicists. For X-ray work a steady direct current was of the greatest importance, especially for those workers who wished to be able to obtain electrons moving in a tube with a definite velocity. For this work a constancy of about ½ per cent. was required. He believed this had been accomplished in America with quite large powers

Captain TUCKER said he had used valves in conjunction with microphones, and hoped to develop this work so as to render the microphone more effective.

Dr. D. OWEN said he had found an examination of the numerical example at the end of the Paper most enlightening. It helped one to realise that the tube was only controlling the energy which was supplied by the generator, only a small proportion being used up in the tube. Could Prof. Fortescue tell us the exact amount in the present case ?

Mr. GOSSLING also commented on the simplicity of the physical phenomena in thermionic valves. When the valve was working properly a stream of electrons passed from the hot filament to the anode, subject to no resistance other than their own repulsions. If there was gas enough in the tube to produce appreciable departure from this simple condition the valves gave trouble. The pressure in a valve was of the order 10^{-4} mm., and the mean free path of an electron at such a pressure was about 10^4 cm., so there was little chance of a collision between an electron and a gas molecule. The rectification by the valves was of a very high order, the reverse current under normal circumstances being about a ten-thousandth of the direct current. If the anode be raised to a high temperature and more gas than usual be present, the fraction may rise to about one in a thousand. In connection with the life of a filament, he thought one of the figures given by the author viz., 2,000 hours at 100 watts per ampere—was rather a rosy estimate. The second figure given was probably nearer the case.

Mr. RAYNER said that in many cases where there was some voltage in excess of that required, an effective method of smoothing out "ripples" in the direct current was to replace the condenser C (Fig. 1) by a number of smaller condensers in parallel, separated by resistances.

Mr. COURSEY stated that choking coils had often been used in place of the resistances suggested by Mr. Rayner, and had been found effective. What was the basis of the statement that the dimensions of the condenser were proportional to CV ? He would have thought they were proportional to CV^2.

Prof. FORTESCUE, in reply, said he had scarcely indicated in the Paper the extent to which we were indebted to Langmuir and other American workers in connection with the development of valves. At least 50 per cent. of our knowledge came from America. In reply to Dr. Owen, the efficiency was about 60 per cent. under the conditions of the demonstration. Mr. Rayner's suggestion was excellent, and had already been widely applied, as had also the use of choking coils. The beauty of the scheme was that if the first resistance and capacity reduced the alternating component of the current in the ratio $1/a$, the next reduced it to $1/a^2$, &c. Mr. Coursey's criticism was correct. The product CV_0^2 was proportional to the overall dimensions, and not the product CV_0, as stated in the proofs that had been circulated.

XXIV. *On the Measurement of Small Susceptibilities by a Portable Instrument.* By *Prof.* ERNEST WILSON.

RECEIVED MAY, 20, 1919.

IN a Paper on "The Magnetic Balance of MM. P. Curie and C. Chéneveau," * M. Chéneveau describes an instrument which involves the same fundamental principles as the portable instrument which is the subject of the present communication. It is capable of dealing with the measurement of very small susceptibilities, and its indications are recorded on a scale after the manner of an ordinary reflecting galvanometer. It employs a permanent magnet of ring form, which gives a constant intensity to the magnetic field.

In connection with some recent work on rock specimens† the author had occasion to develop an instrument for the measurement of susceptibilities of low order, which was described in a Paper read before the Royal Society on January 29, 1919. This instrument differs from that of MM. Curie and Chéneveau in that an electro-magnet is employed instead of a permanent magnet, and thus the magnetic field can be varied in intensity. It appeared desirable to construct a more simple instrument, which would give the desired sensitivity and at the same time should be portable and capable of easy and rapid adjustment.

For magnetic survey work the important range of susceptibilities is from about 0·000002, as in the case of limestones and dolomites, for example, to 0·15, as in the case of low-grade magnetities, the superior limit being about 3 or 4 in the case of magnetite crystals. The instrument to be described is capable of measuring from about 0·15 to 0·0003 with a strong suspension, and so low as 0·000015 with a weaker but sufficiently strong suspension, and indications of lower order than 0·000015 can be observed. Other illustrative cases of rock specimens are certain dolorites, 0·0045; ordinary grey granite, 0·001 to 0·002; red hæmatite, 0·0002; ironstone, 0·0003; and ruby mica in the direction of the laminations, 0·000012.

The principle underlying the action of the instrument is given by Maxwell in the equation

$$F=\frac{1}{2}K\frac{dH^2}{dx},$$

where F is the mechanical force per unit volume acting upon

* "Proc." Phys. Soc. Lond., 1910, Vol. XXII., Pt. III., p. 343.
† "Phil. Trans." R.S. A, Vol. 219 (Appendix), 1919.

the substance, whose susceptibility is K, and dH^2/dx is the gradient of the square of the magnetic force varying with x the distance. The specimen is preferably in the form of a short cylinder, or when in the form of powder or solution is contained in a small glass tube, and is fixed to one end of a horizontal beam, which is supported by a phosphor-bronze strip. Normally it hangs between two fixed poles of wrought iron, which are capable of being magnetised to varying degrees of strength by a permanent magnet. The torsion-head of the instrument which supports the suspended system is turned until the angle of twist between it and the beam is a maximum. Let this angle be θ, and let the volume of the specimen be V,

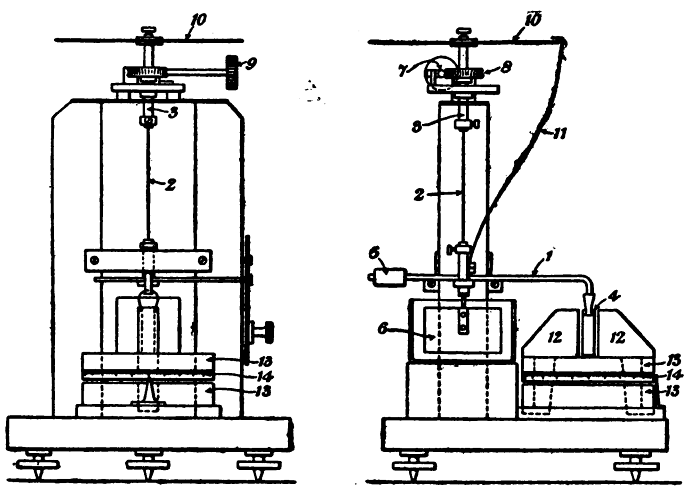


FIG. 1.

then the susceptibility K is calculable from the equation $K=C\theta/H^2V$, where H is the intensity of the magnetic field and C is a constant of the instrument.

The Instrument.

The instrument is shown in front and side elevation in Fig. 1. The horizontal beam, 1, is supported by a phosphor-bronze strip, 2, attached to the torsion-head, 3. The specimen 4 is held in a grip at one end of the beam 1, and is counterweighted by a sliding weight, 5. A vane, 6, submerged in oil serves to damp out the oscillations of the moving system, and

thus enables readings to be quickly made. Although more convenient, magnetic damping was not employed, as it was thought inadvisable to have any possibility of disturbance when small magnetic fields were used. By means of a worm, 7, and worm-wheel, 8, the torsion-head 3 can be gradually turned in either direction, a milled head, *a*, being provided on an extension of the worm-wheel spindle. The torsion-head carries a disc, 10, which is provided with a scale divided into 360 equal parts, and the pointer 11 attached to the moving system indicates the angular movement of the torsion-head from its normal or zero position. The specimen 4 hangs between the poles 12, 12, which are fixed to the base of the instrument. The magnet consists of two rings of tungsten steel, 13, 13, capable of rotation in a horizontal plane about an axis coinciding with the axis of the specimen, 4, when hanging symmetrically between the pole-pieces. Each ring is magnetised with north and south poles on opposite ends of a diameter, and the upper ring is provided with a scale, 14, in order that the relative position of the poles on the magnet rings can be determined. If the magnet rings are so placed that their like poles are together and coincide with the poles 12, 12, one of these becomes a north and the other a south pole, and in this case the intensity of the magnetic force between the poles is a maximum.

Two methods for variation of the force H are now available (*a*) keeping the like poles together by means of the locking device, the two rings can be turned through known angles and the force passes through zero value when the angular movement is 90 deg. from the maximum; (*b*) keeping the lower magnet ring fixed in the position of maximum force and turning the upper ring from its position of maximum force, the value of H can be reduced until the angle turned through is 180 deg., when the difference between the two magnets is obtained. In practice it is difficult to obtain an exact equality in the strength of the two rings, and, moreover, the difference between two large and nearly equal forces is liable to be seriously affected by a slight variation in one of them. During the ageing of the magnets, therefore, this difference is liable to vary. For this reason it is desirable to vary the magnetic force by turning the two rings simultaneously, thus sacrificing the more open scale available when one ring only is rotated. The second method is used for demagnetising purposes to be described later.

The Magnet Rings.

Each ring has internal and external diameters of 3½ in. and 4¼ in. (8·9 cm. and 10·8 cm.) respectively, and its depth is ¾ in. (1·9 cm.). The material is of hardened tungsten steel, and in this connection the author is indebted to Mr. W. Carter, Messrs. J. J. Saville (Ltd.), Sheffield, for supplying these rings and for the interest he took in their preparation. A yoke piece of wrought iron having a cross-section 1½ in. by ½ in. (3·8 cm. by 1·27 cm.) was wound with a magnetising coil of 225 turns and bridged across a diameter of the two rings when they were finally being magnetised, so as to produce opposite poles. The magnetising force due to this yoke was reinforced by windings of 124 turns on either side of the rings.

With the yoke removed the two windings on the rings were placed in series and used as a primary, the total turns being 248, for the purpose of ballistic galvanometer tests. as it appeared desirable to find the magnetic properties of the rings. Secondary windings were placed on the rings and yoke piece. The results obtained are summarised in the following table, and are illustrated in Fig. 2. They indicate that this steel is of the highest grade for the purpose of permanent magnets :—

$H_{max.}$	$B_{max.}$	Coercive force H_0.	Retained magnetic induction B_0.	Hysteresis. Ergs per cycle per cubic cm.
152	13,700	56	10,300	213,000
95	11,800	52	8,370	151,000
74	7,430	42	4,570	70,100
36	1,770	5	250	1,990
20	840	2	62	398

The above figures were obtained from experiments with the first two rings. A second pair of rings was also available, but these were not tested for hysteresis loss. A current of 15·2 amperes was used for the highest force in the above tests, and with the yoke in position the current was put up to 20 amperes and gradually removed. In the case of the second set of rings the final current with yoke in position was 30 amperes.

The ageing of these rings is one of the vital points in the instrument. It was determined by measuring the force between the poles of the magnet. For this purpose a coil having the same dimensions as the poles was attached to a ballistic galvanometer, and deflections were obtained on quickly

removing it from the gap. After much experimenting the first set of rings has aged 31·5 per cent. from March 14th to May 19th, and the second set has aged 39 per cent. It remains to be seen if this particular method of producing a variable field is successful from the standpoint of ageing. Experiments have been made with other arrangements of magnets and pole-pieces and these are being further investigated.

Demagnetisation.

When it is desired to demagnetise the pole-piece or a specimen after the application of a large force the rings were locked relatively to one another at intervals gradually increasing the angular displacement between the poles, and thus diminishing the maximum field. At each interval the two rings, so locked together, were rotated a few times. A series of curves was obtained showing how the force varied during each operation. The curves show a gradually diminishing amplitude combined with variable phase displacement.

The Pole-pieces.

From the standpoint of convenience in working with the instrument the type of poles shown in Fig. 1 is satisfactory, but experiments with various types show that the maximum magnetic force for a given pair of rings and given gap between the pole-pieces can be increased by using two poles which are in the plane of the rings and not elevated as shown. The gain is considerable, and amounts to about 30 per cent., the difference being caused by leakage. For this reason the latter type of poles has been adopted. They are cut from a bar of good wrought iron $1\frac{1}{2}$ in. by $1\frac{1}{2}$ in. (3·8 cm. by 3·8 cm.) in section. The width where they come in contact with the inner circumference of the rings is curved to a radius of $1\frac{3}{4}$ in. (4·4 cm.), and the fit is a good one. The width of the pole-piece itself is $\frac{1}{2}$ in. (1·27 cm.), and the corners are slightly bevelled. The gap length is 1·26 cm. Tests made with the exploring coil show that after the application of a force in the gap (H) of 327 C.G.S. units the magnetism retained when the rings are removed is $2\frac{1}{4}$ per cent., but this could be removed by the method above described.

The Phosphor-Bronze Suspension.

Two sizes of phosphor-bronze strip have been used. The strong one has a rectangular cross-section of 0·85 mm. by

0·09 mm., and its length is 10 cm. At a radius of 10·9 cm. the force in dynes per 1 deg. of twist was measured, and found to be 1·50. This strip does not take up an appreciable permanent set when one end is twisted through 180 deg. The smaller section of strip is 0·85 mm. by 0·04 mm., and experi-

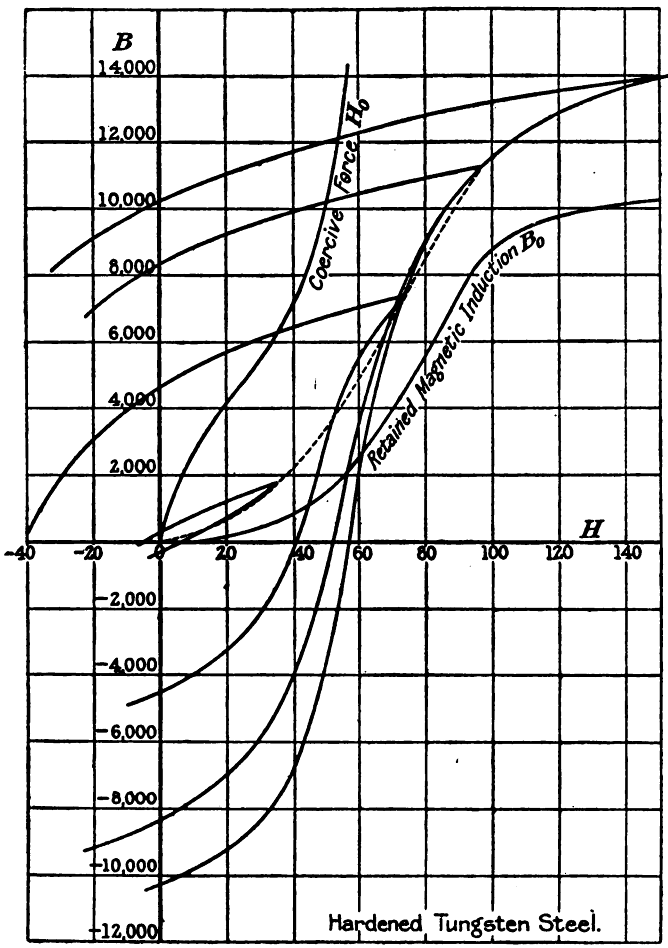


Fig. 2.

ment shows that the ratio of the restoring forces for equal lengths is 20·6 to 1 in the two cases. With this smaller section of strip the observed deflection is slightly dependent upon loading due to heavy and light specimens.

The Constant of the Instrument.

From previous experiments with the more elaborate instruments several specimens of widely differing susceptibilities were available. They are not truly circular in section, as when dealing with rock specimens much time would be spent in making them so. Table I. gives some of the figures obtained with the two suspensions with the magnet rings in the position of maximum force, and it will be seen that the ratio of the instrumental constants is about the same as that of the restoring forces in the suspensions. The considerable variation in the value of the constant is due in part to the fact that the specimens are not exactly alike as regards shape and size. Also in the case of rock specimens the susceptibility is liable to vary throughout the mass (*loc. cit.*).

Variation of H with Angular Position.

The variation of the magnetic force H with angular movement of the two magnet rings, locked together with their like poles coincident, was tested by the exploring coil and ballistic galvanometer, and also by the deflections obtained with two specimens in the gap. In Table II. values of H so obtained are compared with the values calculated on the assumption of the sine law. The agreement is such that given the maximum value of H, the force for other positions can be calculated. It is thus possible to apply known forces when making determinations, and this is important in some cases in which the susceptibility is not a constant, and is some function of H. But even when the susceptibility is constant it is necessary for a given suspension and large range to vary the force; that is to say, the phosphor-bronze strip must not be twisted beyond the elastic limits, and, therefore, when the susceptibility is large a small value of H is imperative. It will be seen that the dolerite mentioned above necessitated a twist of 160 deg., when the value of H was at a maximum. For higher susceptibilities it would be advisable to reduce the field or the volume of the specimen, the former being preferable.

The Use of Powders.

If rock specimens are ground in a non-magnetic mortar, and then tested for susceptibility, in a large number of cases the susceptibility of the original rock can be fairly closely inferred

TABLE I.

Description.	Instrumental readings.		Mean value $\theta°$.	Previous determination of susceptibility.	Instrumental constant C.
	Left.	Right.			
Strong Suspension—					
Grey granite	91	98	94·5	0·0021	7·75
Ironstone	11	11	11·0	0·0003	9·80
Chalybite	40·5	41·5	41	0·0006	9·5
Red hæmatite	15	16	15·5	0·00022	9·5
Dolerite	156	164	160	0·0047	8·4
Weak Suspension—					
Kaolin and chalybite mixture unbaked	103	92	97·5	0·000153	0·50
Manganese sulphate, normal solution	13·5	6·5	10	0·000018	0·43

TABLE II.

Sine curve.			Ballistic test. First set of rings.		Ballistic test. Second set of rings.		Grey granite, $K=0·0021$.			Kaolin-Chalybite, $K=0·000153$.		
Angle.	Cosine.	$H_{max.}$ 345.	Deflection.	H.	Deflection.	H.	$\theta°$.	$\sqrt{\theta°}$.	H.	$\theta°$.	$\sqrt{\theta°}$.	H.
0	1	345	305	345	413	345	53	7·28	345	99	9·95	345
15	0·966	333	300	339	406	339	51·25	7·15	340	97·5	9·87	340
30	0·866	298	267	302	365	305	40·25	6·34	300	82	9·05	314
45	0·707	244	220	249	297	248	26·5	5·15	244	53·75	7·33	254
60	0·500	172	165	187	222	186	13·5	3·67	174	27	5·2	180
75	0·259	89·5	98	111	131	109	3·5	1·87	88·5	9·75	3·12	108
90	0	0	+30	34	+44	37	0	0	0	0	0	0
97·5	...	...	−11	12·4	−6	5	...	...	...	...	...	...

(*loc. cit.*). In magnetic survey work it is important to test a large number of specimens of the same rock in order to obtain an average. It may therefore be an advantage to grind such specimens, as the work entailed by cutting to shape would be prohibitive. When powders are used they are contained in a small glass tube which has a diameter of about 1 cm. and a length of about 4 cm., correction being made if necessary for the glass in the tube.

SUMMARY.

A horizontal beam is supported by a phosphor-bronze strip attached to a torsion head. The specimen is held in a grip at one end of the beam, and is counterpoised by a sliding weight. The specimen hangs between pole-pieces which are fixed to the base of the instrument. The magnet consists of two rings of tungsten steel, capable of rotation in a horizontal plane about an axis coinciding with the specimen. Each ring is magnetised with north and souch poles at opposite ends of a diameter. By varying the relative azimuths of the rings the field between the pole-pieces can be varied. The method of test consists of turning the torsion head until the specimen just breaks away from the field between the poles. If the torsion angle be θ and the volume of the specimen be V, the susceptibility K is given by $K=C\theta/H^2V$, where H is the intensity of the field and C is a constant.

DISCUSSION.

Mr. R. S. Whipple thought the author's method of varying the field by means of rotating ring magnets was very ingenious. In testing the parts of moving-coil instruments for traces of magnetism they had found it very valuable to take the time of swing in a magnetic field and in a zero field. He was inclined to think that this was the simplest and possibly most accurate method of measuring small susceptibilities.

Mr. J. Guild suspected that the instrument would be particularly liable to zero changes due to subpermanent sets in the suspension.

Prof. Lees asked if with different specimens the maximum torsion occurred when the specimen was at the same point of the field. Unless this was so, dH^2/dx would not be constant, as required by the theory. Arising out of this point, what was the effect of the actual size of the specimens on the results, as dH^2/dx would not have the same value for large and small specimens.

Prof. Wilson, in reply to Mr. Whipple, said that if you had two materials of the same susceptibility but of different electrical conductivities, the times of swing would, he thought, be seriously affected by the difference in the eddy currents set up in the two cases. In reply to Mr. Guild, he had found no zero change effects after torsions of 180 deg. With regard to the President's points, it was necessary in order to get consistent results to use specimens all about the one size. As regards the position in the field at which the specimen breaks away, this did vary slightly with the strength of the field; but he was not sure that this would appreciably affect the torsion, which was what the instrument actually measured.

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P.

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S.

T.

V.

W.

End